Essays on financial education and behavioral household finance

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To Laura

Contents

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Overview of papers and co-authorship

Papers included in this dissertation are:

(i) Kaiser, Tim and Lukas Menkhoff (2016). Does financial education impact financial behavior, and if so, when? *DIW Discussion Paper 1562*.

(Resubmitted to *World Bank Economic Review* after minor revision.)

(ii) Kaiser, Tim (2017). Experimental evidence on the causal effects of financial education among small-scale retailers in rural Uganda. *Mimeo.*

(iii) Kaiser, Tim und Vera Kirchner (2015). Das Finanzwissen angehender Wirtschaftslehrpersonen: Ergebnisse eines aktuellen Surveys. *Zeitschrift für Berufs- und Wirtschaftspädagogik*, 111(4): 552–574.

(iv) Birke, Franziska, Kaiser, Tim und Andreas Lutter (2015). Schülerkonzepte zu ordnungspolitischen Fragen – Eine phänomenographische Untersuchung. *Zeitschrift für Didaktik der Gesellschaftswissenschaften*, 6(1): 92–110.

General Introduction

Over the course of the past twenty years, globalization and the integration of financial systems have led to an increased complexity in financial markets. Living in such a globalized market-economy requires individuals and households to be equipped with a certain set of knowledge and skills to make well-informed and thoughtful decisions.

In developing economies, the integration of financial systems has led to financial inclusion of populations that have little experience with the complexity of formal financial products. Many fear that these developments may outpace the capacity of individuals and households to make informed financial choices (cf. Bruhn et al 2016, p.256). Unfortunately, evidence from an abundance of survey-data seems to suggest that individuals may indeed lack the financial literacy necessary to deal with these complexities (cf. Lusardi and Mitchell 2014).

Thus, policymakers have embraced financial education programs as an antidote to the increasing complexity of consumers' financial decisions in both developed and developing countries (cf. Fernandes et al. 2014). Such policies are seen to be crucial not only to improving individual and household welfare, but achieving greater stability of the financial system (cf. Carpena et al. 2015). Despite the desired benefits of greater financial education, the empirical evidence supporting such presumptions appears to be very limited. While a correlation between higher financial literacy and better financial behaviors has been observed in many empirical studies (see Hastings et al. (2013) and Lusardi and Mitchell (2014) for reviews), credible evidence for a causal impact of financial education programs on financial literacy and behavior appears much more muted. Narrative literature reviews are generally inconclusive, either emphasizing the effectiveness of financial education policies (e.g., Fox et al. 2005; Lusardi and Mitchell 2014) or emphasizing the opposite (e.g. Willis 2011). Further, two meta-analyses of the literature do not converge in their findings: Fernandes et al. (2014) summarize overall zero-effects of financial education, whereas Miller et al. (2015) show that education can be effective in targeting specific financial behaviors. Fortunately, the recent years have seen a steep increase in rigorous evidence generated through randomized controlled trials. Thus, this dissertation seeks to add empirical evidence to the literature in two ways:

Part I of the dissertation tries to advance the understanding of the causal effects of financial education. Thus, it makes two contributions to the economic literature on financial education by combining a quantitative review of the experimental evidence of financial education with a large-scale randomized field experiment in rural Uganda. The first paper is titled "Does financial education impact financial behavior, and if so, when?" and is joint work with Lukas Menkhoff. We synthesize the large body of experimental work on financial education in a recent and comprehensive meta-analysis. While we worked on the manuscript and the research idea together, coding of the numerous studies and data analysis was my responsibility. The paper has been published as *DIW Discussion*

Paper No. 1562 and has recently been resubmitted to the *World Bank Economic Review* after a minor revision.

The second paper is titled "Experimental evidence on the causal effects of financial education among small-scale retailers in rural Uganda". In this paper, I study the differential causal effects of two variants of a financial education program offered to small-scale retailers in rural Western Uganda. Thus, this paper adds new evidence to the body of work concerned with causal impacts of these types of policies. This manuscript has been written in single-authorship, and I have been in charge of the experimental design, field activities, data-collection, and data analysis.

Part II of the dissertation is titled "Research on economic and social science education" and seeks to contribute to the discourse in the field of research on economic and social science education. While similar research questions are relevant in this field, the discourse is different from the discourse in economics (cf. Kaiser and Lutter 2015). As an applied science, research on economic and social science education is mostly interested in studying the determinants and outcomes of teaching and learning in economics and the social sciences. In economic research, the focus lies primarily on a relatively narrow definition of consumer financial literacy. In the field of social sciences education, more inclusive concepts of economic and civic literacy are advocated and understood to incorporate elements of financial literacy (see Remmele and Seeber (2012) and Davies (2015) for articles on the conceptual and normative foundations of integrative concepts that combine financial, economic and social education). While the contributions in part I of the dissertation are also relevant to the current discourse on impact evaluation in this type of educational research, the main discourse in the discipline is still related to understanding the foundations of teaching and learning in the domains of economics and social sciences.

Thus, the third paper "Financial knowledge among future economics teachers: Results from a recent survey" explores the determinants of financial knowledge among a sample of prospective economics teachers in Germany. This paper is joint work with Vera Kirchner, who has been in charge of data-collection and contributed the review of evidence on teacher professionalism to the manuscript. I have been responsible for data-analysis and writing of the empirical sections of the manuscript. The paper has been published in *Zeitschrift für Berufs- und Wirtschaftspädagogik*.

Finally, the fourth paper is titled "Student conceptions of regulatory policy issues – Results from a phenomenographic study" and has been published in *Journal for Didactics of Social Sciences (ZDG)*. This is joint work with Franziska Birke and Andreas Lutter. I have been responsible for the qualitative research design, conducting the interviews, and initial analyses of the qualitative data. Final interpretations and the manuscript are joint work of all authors.

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Part I: Causal effects of financial education

(i) Does financial education impact financial behavior, and if so, when?

Tim Kaiser and Lukas Menkhoff*

DIW Discussion Paper No. 1562.

Abstract

In a meta-analysis of 126 impact evaluation studies, we find that financial education significantly impacts financial behavior and, to an even larger extent, financial literacy. These results also hold for the subsample of randomized experiments (RCTs). However, intervention impacts are highly heterogeneous: Financial education is less effective for low-income clients as well as in low and lower-middle income economies. Specific behaviors, such as the handling of debt, are more difficult to influence and mandatory financial education generally appears to be less effective. Thus, intervention success depends crucially on increasing education intensity and offering financial education at a "teachable moment."

JEL-Classification: D 14 (personal finance), I 21 (analysis of education)

Keywords: Financial education, financial literacy, financial behavior, meta-analysis, meta-regression, impact evaluation

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Does financial education impact financial behavior, and if so, when?

1 Introduction

The financial behavior of consumers and small-scale entrepreneurs is receiving increased interest. Evidence suggests a remarkable incidence of suboptimal individual financial decisions, despite the fact that these decisions are highly relevant for individual welfare. The most prominent case of such an important financial decision in advanced economies is the amount and kind of retirement savings (cf. Duflo and Saez 2003). Studies show that under-saving is prevalent in many advanced economies and that households tend to save in inefficient ways, indicating that many may be unable to cope with the increasingly complex financial markets (e.g., Lusardi and Mitchell 2007; Choi et al. 2011, Behrman et al. 2012; van Rooij et al. 2012). This kind of behavior also stretches across other areas, including portfolio composition (Campbell 2006; Choi et al. 2010; Bucher-Koenen and Ziegelmeyer 2014; von Gaudecker 2015), excessive and overly expensive borrowing (Stango and Zinman 2009; Gathergood 2012; Agarwal and Mazumder 2013; Gerardi et al. 2013; Zinman 2015), as well as participation in financial markets in general (van Rooij et al. 2011). Related problems arise in developing countries, often with even more serious consequences as people are exposed to heavy shocks without having sufficient insurance or mitigation instruments (e.g., Cole et al. 2011; Drexler et al. 2014; Gibson et al. 2014; Sayinzoga et al. 2016). All this strongly motivates providing financial education to foster financial behavior.

In surprising contrast to this obvious motivation for financial education stands the lack of compelling evidence that providing financial education is an effective policy for targeting individual financial behavior (Hastings et al. 2013; Zinman 2015). Narrative literature reviews are inconclusive, either emphasizing the effectiveness of education measures (e.g., Fox et al. 2005; Lusardi and Mitchell 2014) or emphasizing the opposite (e.g. Willis 2011). Further, the two available systematic reviews of this issue, both applying a meta-analysis approach, do not converge in their findings: Fernandes et al. (2014) summarize overall unreliable effects of financial education, whereas Miller et al. (2015) show that education can be effective in targeting specific financial behaviors. Given this inconclusive evidence on a most important issue, what

can we learn in order to explain the heterogeneity in findings and to make financial education more effective?

Our main contribution is analyzing the heterogeneity around a small positive average treatment-effect of financial education. Thus, we go beyond the extant literature and systematically code the circumstances of financial education for our meta-analysis. This allows us to examine the determinants of a positive impact of education. Another unique characteristic of our analysis is the focus on both objectives of financial education, i.e. improvements in financial behavior and in financial literacy. Hence, we investigate the role of financial literacy for financial behavior in a unified setting. Finally, our study benefits from a rapidly rising field, as indicated by the increased number of citations of publications using the keyword "financial literacy" (see Figure A1 in Appendix A). Beyond the number of studies, the quality is also improving due to rigorous impact evaluation methods, which allow for a more precise estimation of treatment effects.

We follow the established procedures for the meta-analysis approach (e.g. Lipsey and Wilson 2001). This means that we describe how we searched for relevant studies and how we chose selected studies to avoid biases. The result is a sample of 126 studies reporting 539 effects of financial education on financial literacy and financial behaviors of individuals. Studies exclusively targeting entrepreneurs and measuring business outcomes are omitted by design. We only consider studies reporting about interventions, such as trainings and counseling efforts. Thus, we focus strictly on exogenous variation in financial education and neglect works exclusively analyzing the possible impact of cross-sectional (baseline) differences in financial literacy on financial behavior. Finally, we carefully code interventions as we examine in detail how financial education was delivered to the target groups.

The main finding of our meta-analysis is that financial education does indeed impact financial behavior in the intended way. However, the way financial education is provided is crucial because its unconditional effectiveness is small. The average impact of financial education on all reported outcomes, i.e. the average effect size, is 0.15; the impact on financial behavior is, at 0.09, even smaller. These effects are statistically highly significant and robust, but they are clearly below the threshold value of 0.20 that characterizes 'small' statistical effect sizes (see Cohen 1977). Thus, it seems important to learn what might increase program effectiveness in the future from earlier work.

Our meta-analysis results in six principle findings: (i) Increasing *financial literacy* helps. Financial education has a stronger positive impact on financial literacy (effect size of 0.26) than on behavior, while effect sizes on financial literacy are positively correlated with effect sizes on financial behavior; (ii) financial education has a positive, measurable, *impact on financial behavior*. Effectiveness is still found under rigorous evaluation methods, such as randomized experiments (RCTs); (iii) effects of financial education depend on the *target group*: first, teaching low-income participants (relative to the country mean) and target groups in low- and lower-middle income economies has less impact, which is an obvious challenge for policymakers targeting the poor, second, it appears to be challenging to impact financial behavior as country incomes and mean years of schooling increase, probably because high baseline levels of general education and financial literacy cause diminishing marginal returns to additional financial education; (iv) success of financial education depends on the *type of financial behavior* targeted. We provide evidence that borrowing behavior may be more difficult to impact than saving behavior by conventional financial education; (v) increasing *intensity* supports the effect of financial education; and (vi) the *characteristics* of financial education can make a difference. Making financial education mandatory is associated with smaller effects. By contrast, a positive effect is associated with providing financial education at a "teachable moment," i.e. when teaching is directly linked to decisions of immediate relevance to the target group.

Complementing these findings, the meta-analysis also provides interesting non-results because several characteristics of financial education are without systematic impact on financial behavior. These include the age and gender of participants, the setting, or the choice of intervention-channel through which financial education is delivered.

The findings reported above clearly motivate the need to implement financial education because it can positively affect financial behavior. However, its limited effectiveness raises two additional problems for policymakers: First, what can be done to make financial education generally more effective? Second, as a particularly obstinate aspect of the general question raised before, how can one reach those people who do not participate voluntarily? Problematic groups in this respect include low-income individuals, residents of low-income countries, and all those who do not self-select into education measures, as indicated by negative effects from mandatory courses and RCTs. For these groups, it appears that financial education needs an

improved approach to be successful. More research and experience is necessary to better identify the determinants of successful financial education (e.g., Hastings et al. 2013).

Our study follows several earlier survey studies about financial literacy and closely related issues. Most of these studies have a narrative character, among them widely cited works such as Fox et al. (2005), Willis (2011), Hastings et al. (2013), and Lusardi and Mitchell (2014). This gives the authors some flexibility about selecting and interpreting the most relevant studies. A quantitative meta-analysis is more rigid in approach but has the advantages that transparent rules of procedure ensure fully replicable results and that quantitative relations can be derived. Overall, narrative surveys and meta-studies complement each other.

We perform a meta-analysis because there are just two earlier systematic accounts of the financial education literature that leave much room for more research. The study by Miller et al. (2015) covers only 19 papers due to its extremely restrictive selection criteria, requiring interventions on *identical* outcomes. This limits the sample sizes to about five studies and estimates per subsample, which does not allow investigating the sources of heterogeneity.

Thus, the most similar study to our work is Fernandes et al. (2014), which covers 90 effect sizes from financial education reported in 77 papers. Despite an overlap of 44% with their sample of studies, our research differs in four crucial ways, which explains our new results: (i) most important is that we analyze determinants of program effectiveness in a broader way by applying respective coding. (ii) Then we consider the various outcomes per study (on average about four per study) and their respective effectiveness. Moreover, (iii) we cover recent and mostly randomized experiments providing evidence of effective interventions; and (iv) we cover additional studies focusing exclusively on financial literacy as the outcome variable.

This paper is structured in seven further sections. Section 2 introduces our meta-analytic approach. Section 3 describes our data. Section 4 provides first results of the meta-analysis, while Section 5 uses these results to explain heterogeneity of financial education treatment effects. Robustness tests are mentioned in Section 6 and Section 7 concludes with policy considerations and venues for future research.

2 Meta-analytic method

Meta-analysis is a quantitative method to synthesize findings from multiple empirical studies on the same empirical research question. In a meta-analysis, the dependent variable is comprised of a summary statistics reported in the primary research reports, while the explanatory variables may include characteristics of the research design, the sample studied, or in case of impact evaluations, the policy intervention itself (cf. Stanley 2001, p.131). Meta-analyses can provide answers to two specific questions that are highly relevant in contested economic literatures (cf. Muller 2015; Pritchett and Sandefur 2015; Vivalt 2015): First, is the combined (statistical) effect across all studies reporting effects of similar interventions on similar outcomes significantly different from zero? And, second, what explains heterogeneity in the reported findings?

In order to be able to aggregate summary statistics reported across heterogeneous studies, one must standardize these statistics into a common metric. Ideally, all studies would operationalize and measure outcomes in the same way (i.e. in the same unit). If this was the case, meta-analysis could be performed directly using *economic* effect sizes (e.g. elasticities or marginal effects) in contrast to *statistical* effect sizes (cf. Stanley and Doucouliagos 2012, p.23). This, however, is rarely the case in a large sample of heterogeneous (quasi-) experimental impact evaluations.

Thus, we use a standard approach of coding a variable capturing intervention success and impact. Our impact measure (effect size) is the standardized mean difference (SMD) for each treatment effect estimate. We use the bias corrected standardized mean difference (Hedges' g) as our effect size measure, which is defined as the mean difference in outcomes between the treatment (M_T) and control (M_C) (i.e. the treatment effect) groups as a proportion of the pooled standard deviation (SD_p) of the dependent variable:

$$g = \frac{\mathrm{M}_T - \mathrm{M}_\mathrm{C}}{\mathrm{SD}_\mathrm{p}} \tag{1}$$

with

$$SD_p = \sqrt{\frac{(\mathrm{n}_\mathrm{T}-1)\,SD_T{}^2 + (\mathrm{n}_\mathrm{C}-1)\,SD_C{}^2}{n_T{}^2 + n_C{}^2 - 2}}. \tag{2}$$

where n_T and SD_T are the sample size and standard deviation of the treatment group, and n_C and SD_C are for the control group. Additionally, we capture the standard error of each standardized mean difference (g), which is defined as:

$$SE_g = \sqrt{\frac{n_T+n_C}{n_T n_C} + \frac{g^2}{2(n_T+n_C)}} \quad (3)$$

Hedges' g informs about the size and direction of an effect in scale-free standard deviation units. This metric is only slightly different from other popular effect size measures in experimental impact evaluations, such as Cohen's *d* and Glass Δ (see, e.g., Banerjee et al. 2016). Hedges' g, however, introduces minor corrections that reduce bias in the effect size estimate in cases with small sample sizes and when the sample sizes of treatment and control groups are unequally distributed. When operationalizing effect sizes using alternative measures or converting to (partial) correlations, we do not find significant differences in results (cf. Lipsey and Wilson 2001).

As a rule of thumb, Cohen (1977) suggests that effect sizes smaller than 0.20 should be considered as a "small effect;" effect sizes around 0.50 indicate a "medium effect;" while effect sizes greater than 0.80 constitute "large effects." Where pure mean comparisons, standard deviations and sample sizes for each experimental outcome are not reported directly we exhaust all possibilities to calculate or estimate effect sizes (g) and its corresponding standard error from the range of available statistical data, including regression coefficients and t-statistics (cf. Lipsey and Wilson 2001, p.198).

In the estimation of summary effects of the literature, our main approach to communicate the key results follows a full pooling least squares meta-regression framework, as previously applied in other economic studies (e.g., Card et al. 2015). Assuming that a single true financial education treatment effect (g) can be explained by exogenous, observable characteristics, the impact g on an outcome *i,* reported in study *j* is expressed as a linear function

$$g_{ij} = \alpha + x_{ij}\beta + \epsilon_{ji} \quad (4)$$

where $x_{ij}\beta$ is a vector of observable (exogenous) study-level covariates, such as intensity of intervention, α is an intercept, and ϵ_{ji} denotes an error-term independent

from $x_{ij}\beta$. We estimate our models using multiple (possibly correlated) effect sizes per study and account for heteroscedasticity by clustering standard errors at the study-level. We primarily discuss estimation results based on this easily interpretable meta-analytical model using ordinary least squares. Results are not sensitive to a set of changes in estimation strategy (see Section 6 and Appendix C).

3 Sample description

This section describes the selection of studies (Section 3.1), the extraction of effect sizes and study-level covariates (Section 3.2), and types of financial education programs (Section 3.3).

3.1 Selection of studies

We follow the established meta-analytical protocol (cf. Lipsey and Wilson 2001, p.23; Stanley 2001, p.143). This starts with systematically searching the relevant databases, including working papers, for the following keywords: (i) financial literacy; (ii) financial knowledge; (iii) financial education; (iv) financial capability; and (v) combinations of these keywords with "intervention." Moreover, we consider all records from meta-studies (Fernandes et al. 2014; Miller et al. 2015) and narrative literature reviews (Fox et al. 2005; Collins and O'Rourke 2010; Willis 2011; Xu and Zia 2012; Hastings et al. 2013; Blue et al. 2014; Lusardi and Mitchell 2014). This search resulted in over 500 potentially relevant published journal-articles and over 600 results from working paper databases with some apparent overlap. We stopped collecting articles from these databases in October 2016.

From this collection, we drop studies that do not meet our three criteria for inclusion: (i) Reporting on impacts of an exogenous educational intervention on financial literacy and / or financial behavior; (ii) providing a quantitative assessment of intervention impact that allows coding an effect size statistic (g) and its standard error; and (iii) relying on an observed counterfactual in the estimation of intervention impacts. This selection process leads to a final sample of 126 independent intervention studies that report 539 effect sizes (further details in the Table A1). Of these, 90 studies report 349 effect sizes on financial behavior, and 67 studies report 190 effect sizes on financial literacy. Among these 90 plus 67 studies, there are 31 studies reporting effect sizes on both financial literacy and behavior.

RCTs are rare in the early years of the literature, but their share has risen dramatically, with the majority of studies conducted from 2011 onward being randomized evaluations (see Figure 1). This development in the literature is very favorable for meta-analyses, since it ensures a high internal validity of research findings reported in the primary studies, helps to clearly distinguish between selection- and treatment effects, and leads to higher comparability across these studies.

<Figure 1 about here>

3.2 Extraction of effect size estimates and study descriptors

The next step in our meta-analytic process is to extract effect size estimates from the statistical data reported in the primary studies. We code the effect of financial education on *financial literacy* (i.e. a measure of performance on a financial knowledge test) since knowledge development is the primary goal of financial education (Hastings et al. 2013; Lusardi and Mitchell 2014). Moreover, we code treatment effects of financial education on several *financial behaviors* (see Table A2). These desired behaviors include, for example, an increase in savings after the treatment. Multiple estimates per study are considered if multiple outcomes, time-points, or treatments are reported on, however, results are robust to aggregating all effects per study into one effect. Further details about this process, including the use of a coding protocol (see guidelines by Lipsey and Wilson 2001, p.88), are described in Appendix A.

3.3 Types of financial education programs

Our dataset includes four main types of financial education programs. First, and most frequent, are evaluations of *classroom financial education* (approx. 83% of all estimates) in various settings, such as schools, universities, the workplace, or specific sites such as savings groups or microfinance institutions. These studies are quasi experiments or RCTs, in which the researcher has control over content, intensity, and survey design in order to measure specific outcomes. There is an increasing interest in the literature in multiple-treatment and cross-over designs in order to investigate optimal delivery strategies and potential causal mechanisms (i.e. Drexler et al. 2014; Carpena et al. 2015; Skimmyhorn et al. 2016). These studies have high internal validity, but may report site-specific effects that causally interact with unobserved

features of the specific sites. Additionally, measurement of outcomes is typically in the short or medium run (approx. 65%), since long time series are usually not available. A different strand of the literature evaluating this type of program looks at classroom financial education utilizing (plausibly exogenous) variation in (mandatory) school financial education mandates (e.g. Tennyson and Nguyen 2001; Brown et al. 2016). These studies are typically quasi-experimental in nature and, while possibly weaker in internal validity, possess high external validity, since they typically have very large sample sizes and measure relatively long-run effects on behavioral outcomes (such as savings or mortgage default).

A second type of intervention is *online financial education* (approx. 8% of estimates). While similar in research design to experiments on classroom financial education, these studies usually estimate the effect of certain online modules on financial literacy and behavior and typically evaluate instructional videos or interactive applications.

The third type of financial education treatments evaluated in the literature are *individualized counseling interventions* (2% of estimates). These have been mainly studied in the U.S. and typically study outcomes related to the handling of (mortgage) debt.

As a fourth and last type, we identify *informational and behavioral nudges*, such as information-fairs at the workplace and informational brochures (7% of estimates). These studies typically evaluate behavioral change in response to these low-intensity treatments. There is one study in our sample that studies the effect of a behavioral nudge in form of "financial edutainment" in mass-media (cf. Berg and Zia 2013). This is an intervention designed to impact financial behaviors through a non-cognitive channel (as opposed to increasing financial knowledge) and the included study evaluates the impact of financial messages inserted into episodes of a popular television series in South Africa.

4 Results from the meta-analysis

We report the mean effects for all studies (Section 4.1) and then for subsamples: financial literacy and financial behavior (Section 4.2), types of financial education programs (Section 4.3), research designs (Section 4.4), and different country groups (Section 4.5).

4.1 Summary effects of financial education

Here we discuss the average effects of financial education on financial literacy and financial behavior; based thereon, we study the relation between these two outcomes. As a starting point, we note that the summary effect of financial education on all kinds of reported outcomes is estimated to be g=0.148 (p=0.000, n=539). However, heterogeneity in effect sizes is considerably high, thus indicating that outcomes could be disaggregated for meaningful analyses. This further suggests distinguishing between effect sizes on financial behavior and on financial literacy.

Financial behavior. We find that the average impact of educational interventions on financial behaviors is statistically highly significant (g=0.086, p=0.000, n=349) (see Table 1). Although the average treatment effect of 0.086 is small in magnitude, there exists a measurable and robust impact of financial education on various kinds of financial behavior. The main reason that we get a more favorable result than Fernandes et al. (2014) is that we profit from a moderate, positive time-trend. We explain the differences with their study in detail in Appendix B.

Financial literacy. The average impact of financial education on financial literacy is substantially higher (g=0.263, p=0.000, n=190) than the one on financial behavior. Graphically speaking, the distribution for effect sizes on financial literacy is shifted to the right compared to the distribution for financial behavior (see Figure A2). Moreover, financial education explains 1.7% of the variance in financial knowledge and, thus, appears to be only slightly less effective than educational interventions in other domains, such as math and science instruction (cf. Fernandes et al. 2014, 1867).

Relationship between financial literacy and behavior. As we have information from 31 studies about effects on both financial literacy and financial behavior, we can analyze the importance of financial literacy for behavior. The intuition is that increases in financial literacy scores are an important intermediate result in a causal chain expected to lead to behavior change (e.g. Grohmann et al. 2015; Fort et al. 2016). Indeed, we find in a regression with standard errors clustered at the study-level that the effect size on financial literacy is a statistically significant predictor of effect size on financial behavior (b=0.230, SE=0.093, p =0.022).

Thus, an increase of one standard deviation unit in financial literacy scores is related to an average increase of 0.23 standard deviation units of the financial behaviors studied. This result indicates that the impact of an educational intervention on the financial knowledge of participants is an important link in the causal chain that

is expected to lead to behavioral change. However, the non-overlapping confidence intervals of these effect sizes also indicate that these two elements of the causal chain should be analyzed separately when attempting to explain the heterogeneity in effect sizes.

4.2 Effect sizes by type of financial behavior

While our analysis so far shows that financial education interventions have larger effects on financial literacy than on financial behaviors, effect sizes for behaviors may vary depending on the financial behavior studied. Figure 2 shows the average effect size for the seven categories of financial behaviors targeted by the educational interventions in our sample.

<Figure 2 about here>

Average effect sizes for three out of seven categories of outcomes are clearly positive and highly statistically significant at the 1%-level. Additionally, all confidence intervals for the different types of financial behaviors overlap each other, indicating that there are no extreme differences in impacts depending on the specific form of financial behavior targeted. Two things, however, are noteworthy: (i) The average effect size on "budgeting" appears to be higher than those on downstream behaviors; and (ii) effect sizes related to saving and retirement saving appear to be higher than the average effect size of financial education on borrowing behavior: this latter average effect size is small (g=0.02) and insignificant from zero.

Similarly, the average effect sizes for "open bank account" (g=0.003), "insurance" (g=0.05), and "remittances" (g=0.04) are estimated to be small and insignificant from zero. However, it is noteworthy that these average effect sizes are calculated based on information provided in few studies per category. Thus, of all financial behaviors studied, the effect size on borrowing is clearly the smallest precise estimate, indicating that debt-related financial behaviors may be the most challenging to target through financial education.

Overall, these findings correspond to the results provided by Fernandes et al. (2014) and Miller et al. (2015), both also reporting average effect sizes for various financial behaviors, albeit for smaller samples of studies and effect sizes. Qualitatively our analysis confirms the observation by Miller et al. (2015, p.238) that effects on

borrowing are insignificant from zero and that interventions targeting retirement savings appear to be most successful.

4.3 Effect sizes by type of financial education intervention

We form subsamples by the main types of financial education interventions, as discussed in Section 3.3. First, we compare classroom financial education to three types of non-classroom delivery channels (counseling, online financial education, and informational/behavioral nudges). Second, we distinguish between financial education at school and two non-school settings (workplace and other settings). Panel A of Table 1 shows results split by outcomes on financial literacy and financial behavior. While in-person classroom trainings appear to be (unconditionally) more effective than non-classroom delivery channels in increasing financial knowledge, we observe no statistically significant difference regarding financial behavior. Turning to the intervention setting, it appears that interventions in schools are more effective at increasing financial literacy but yield marginally significant smaller treatment effects on financial behavior. However, we note that these indicators are obviously partially confounded with several other relevant variables (e.g. the age of the participants, the delay in measurement, and research design), which indicates the importance of an examination in a multivariate setting (cf. Section 5).

<Table 1 about here>

4.4 Effect sizes by research design

Regarding research design, Fernandes et al. (2014, p.1865) find that weaker research designs lead to inflated effect sizes: 75 quasi-experimental studies showing an average effect of about g=0.068 (r=0.034), while 15 RCTs show an average (statistically insignificant) effect of only about g=0.018 (r=0.009). Thus, the comparison of effect sizes by research design is important in the assessment of the summary effects of this literature.

Panel B of Table 1 compares average effect sizes for financial behavior and financial literacy as a function of research design. When we focus on financial behaviors as outcomes, RCTs show statistically highly significant (unconditional) effect sizes. These are only slightly smaller than for quasi-experiments, indicating that the small but positive significant effects of financial education exist, even under the

most rigorous empirical standards. RCTs also provide a significant positive effect of financial education on financial literacy, and this effect is again stronger than for financial behavior. However, here the difference between RCTs and other designs is sizable and statistically significant at the 1% level.

4.5 Effect sizes by country groups

To investigate another potential source of heterogeneity, we disaggregate our data by country groups. Panel C of Table 1 shows effect sizes for financial literacy and financial behavior disaggregated by country groups as classified by the World Bank based on 2015 GNI per capita. Low-income economies are defined as those with a GNI per capita of $1,025 or less (lower-middle income economies are from $1,026 to $4,035; upper-middle income is from $4,063 to $12,475; and high income is greater than $12,475). We find that effect sizes on *financial literacy* are substantially higher in developed (high income) economies than in developing economies (low income, lower- and upper- middle income economies). This difference is statistically significant at the 1% level. Turning to effect sizes on *financial behavior*, this difference is statistically insignificant in this unconditional comparison but differences between country groups become more nuanced and statistically significant when controlling for other relevant variables (see Section 5.2).

So far, our meta-analysis yields six important findings: (i) financial education has a significantly positive, but small, impact on financial behavior; (ii) unconditional effect sizes on financial literacy are up to four times higher than effect sizes on financial behavior; (iii) impacts on financial behavior have effect sizes with overlapping confidence intervals, but, borrowing behavior may be more difficult to influence than other financial behaviors; (iv) unconditional effect sizes for RCTs are smaller compared to studies relying on less rigorous experimental designs; (v) reported effect sizes on financial behavior do not systematically vary between intervention channels and settings, but may be smaller in school based programs; and (vi) the effect of financial education may be larger in higher income economies.

5 Explaining heterogeneity in financial education treatment effects

Section 4 shows that financial education clearly has an intended effect on financial behavior, and an even stronger effect on financial literacy. However, the average effect is accompanied by large heterogeneity. Thus, we examine whether there

are factors explaining this heterogeneity. This will also suggest directions that future financial education policies might take in order to increase their impact on financial behavior.

5.1 Potential correlates of effect size

The effectiveness of financial education is potentially influenced by the peculiarities of the specific intervention. Based on prior literature, we group these characteristics into four categories: (1) the research design; (2) the intensity of education; (3) the target group of education; and (4) the characteristics of the education program. We describe this full set of potential correlates that may influence the effect size of financial education interventions.

(1) Regarding the *research design* of a financial education study, we expect the method of investigation, i.e. RCT vs. less rigorous designs, to be relevant. Second, the concrete measurement of an effect will influence the estimated size of impact. It is known that focusing on treatment on the treated (TOT), i.e. measuring a treatment effect on the population who actually *received or attended* the treatment, generally results in higher effect sizes than focusing on the intent to treat (ITT) effect, i.e. the population who was in principle *assigned* to treatment. However, ITT may be more relevant for policy (cf. Imbens and Wooldridge 2009, p.15; Gertler et al. 2011, p.73). Third, the delay between financial education treatment and measurement of the effect size may negatively influence the effect size since effects of the intervention may decay over time (cf. Fernandes et al. 2014, p.1867). Additionally, we control for the precision of effect size estimation (by the inverse standard error). All variables motivated and described in this section are defined in Table 2, where descriptive statistics at the study-level are also provided.

<Table 2 about here>

(2) A core variable of financial education interventions, which is usually reported in the papers, is the *intensity of education*, i.e. the number of hours taught. It is expected that higher intensity will support the effect. However, the time-frame over which the financial education intervention is delivered to the target group may also be of importance. We expect differences between high intensity and low intensity relative

to the duration. Thus, we code the *hours of financial education per week* (i.e. intensity per week) and the *duration* of the intervention in weeks to investigate this issue.

(3) The expectation regarding a possible relation between the *target group* of education and effectiveness of financial education – measured by its impact (effect size) on financial behavior – is as follows. Generally, learning is easier for younger people and younger people may be more open to new concepts, meaning that the *age* of the target group may have a negative relation to the effect size of financial education. Also, various empirical studies show that financial literacy is low, especially among the young (e.g. Lusardi and Mitchell 2014), indicating that financial education offered to these participants (with lower baseline scores) may also lead to higher effect sizes in contrast to "older" participants (with relatively high baseline financial literacy). Second, a gender gap in financial literacy is treated as a stylized fact in the literature (cf. Lusardi and Mitchell 2014) which may also translate to gender differences in treatment effects. Thus we include the *percentage of women* in the sample. Third, it is expected that the acquaintance of the target group with an educational environment may be helpful. As a proxy for such openness to education, we take the *income* of the target group relative to the overall population. Fourth, we expect that the overall institutional level of education should support domain-specific educational efforts (Jappelli 2010). As a proxy for this potential relationship, we take a country's population mean *years of schooling* as reported by the United Nations Development Program Human Development Reports. Additionally, we augment our data with *country-level financial literacy* data from a 2015 global financial literacy survey (Klapper et al. 2015). We hypothesize that financial education interventions may yield higher effects when the population baseline financial literacy is lower, indicating more room for improvement through education. Finally, as a control variable we code the country of intervention according to the World Bank country group classifications.

(4) Regarding the *characteristics of the education program*, it seems interesting whether the *channel* (i.e classroom, online, individual counseling, etc.) is important in explaining education effectiveness, since these formats come with different trainer to participant ratios and may rely on different pedagogical approaches to financial education. It may be that willingness to learn and change financial behavior is lower when financial education is *mandatory* (cf. Collins 2013) or motivation to participate in financial education is not intrinsic but driven by *incentives* provided by the offering

institution. Lastly, these characteristics may be correlated with specific *settings* (i.e. at school or at the workplace).

Next, and going further in this direction, it is coded whether participants are educated at a *teachable moment*, i.e. that they have the possibility to apply their knowledge in a concrete case of interest to them (e.g. Doi et al. 2014). Thus, we try to capture whether the provision of education came at a point that addressed immediate financial issues (such as borrowers already in default, or micro entrepreneurs borrowing to extend their business). Alternatively, financial education was generic and offered at an unspecific moment, as is often the case in large scale financial education programs (e.g. Bruhn et al. 2014).

5.2 Meta-regression models explaining intervention impacts

This section examines determinants of financial education effectiveness using a multivariate meta-regression framework including the above discussed potential correlates as right hand side variables. Our procedure is motivated by economic and econometric considerations. From an economic point of view, we aim for including all variables that have a substantial theoretical foundation. From an econometric viewpoint, the specification should be parsimonious, especially in the presence of a relatively small sample size of studies.

Thus we start with a specification where we include all reasonable and available variables (Table 3, column 1). In order to keep the number of studies considered high, we impute average or default values for missing observations (we show in Appendix C that our main results are insensitive to imputation). The discussion considers groups of variables in four blocks, following their introduction in Section 5.1.

<Table 3 about here>

Research design. Starting with the research design of the underlying primary studies, we find that RCTs report – *ceteris paribus* – slightly smaller effect sizes than non-RCTs, which is in line with earlier presumptions (see Table 1, Panel B). However, now this difference is statistically significant. As expected, the operationalization of treatment effects as TOT-estimates leads to inflated effect size estimates. Apparently, the delay between intervention and measurement of outcomes does not seem to be systematically related to effect sizes in this estimation (cf. Appendix C for an

alternative approach and investigation of heterogeneous treatment effects depending on delay in measurement). In addition, estimates with large inverse standard errors are associated with smaller effect sizes, indicating that larger and more precise studies report smaller effect sizes overall. However, this coefficient is small in size and insignificant.

Intensity. Turning to the relationship between intensity per week and duration, column 1 of Table 3 shows that intensity has a significant positive effect on treatment effects on financial behavior. Thus, an increase of one hour of financial education per week leads to a 0.004 standard deviation unit increase in the impact on financial behaviors studied. Considering that the average weekly duration is seven to eight weeks and weekly intensity is only about 3.75 hours, doubling the weekly intensity to 7.5 hours, while keeping everything else constant at the mean, would lead to an average treatment effect around 18 percent higher than the unconditional treatment effect.

Target group. Among participant characteristics, age and gender are not significant explanatory variables. However, the coefficient on 'low income clients' is highly significant and negative, indicating that these individuals are more difficult to educate. Regarding increasing mean years of schooling at the country level, returns to additional financial education appear to diminish. This is in line with results from two studies in very different contexts (Europe and India) that report higher treatment effects for lower-educated individuals and diminishing returns to financial education upon higher baseline levels of education (cf. Cole et al. 2011; Fort et al. 2016). Similarly, the coefficient for baseline financial literacy in the population is also negative, albeit statistically insignificant. While these results suggest declining marginal returns to financial education, the negative effect for low- and lower-middle income economies – and also the above-mentioned coefficient on low-income clients – shows a countervailing influence from challenging groups or country circumstances.

Characteristics of education. Regarding the channel variables, column 1 shows that no alternative channel appears to be generally more or less effective than financial education in classroom settings or informational nudges (omitted category). The same is true for the setting of the intervention where school and workplace settings are not systematically different from other settings. However, mandatory financial education and implementing financial education at a 'teachable moment' appear to be important. Specifically, we find, that making financial education mandatory decreases

effect sizes by 0.074 standard deviation units: The predicted value for effect size on financial behavior in mandatory formats with everything else kept equal at the (empirical) mean would be only g=0.030 (SE=0.020, p=0.134); thus, economically small and statistically insignificant from zero. In contrast, offering financial education at a teachable moment increases effect sizes by 0.079 standard deviation units. Thus, the predicted value for effect size on financial behavior would be *ceteris paribus* g=0.12 (SE=0.014, p=0.000), i.e. roughly 48 percent larger than the unconditional average effect size found in the sample and statistically highly significant.

Parsimonious specification. We reduce the above discussed fully specified model by keeping the variables on research design and intensity but otherwise eliminating the insignificant variables. Column 2 of Table 3 describes the resulting reduced model that confirms the fully specified regression results from column 1. There are just some smaller changes in the estimated standard errors that occur at a few variables. This indicates that it is justified to rely on the parsimonious specification, in particular when we later analyze subsamples with a much smaller number of observations.

5.3 Meta-regression models for subsamples

Given the large degree of heterogeneity across the 90 studies and their underlying financial education programs targeting financial behaviors, we move to an analysis of more homogenous subsamples in the following.

RCTs only. Many will agree that RCTs fulfill the most rigorous requirements implying that results limited to this subsample of studies are indeed reliable. We do not prefer this procedure because many observations are lost. Nevertheless, it is reassuring that results qualitatively hold, as shown in column 3 of Table 3 for the subsample of 40 RCTs covering 227 effect sizes. However, while the negative coefficient for mandatory courses remains to be large in magnitude and statistically (marginally) significant, the coefficient for teachable moment loses explanatory power in this estimation.

Interventions in low and lower-middle income economies. This subsample covers 18 studies that report 129 effect sizes. Again, all coefficients have the same sign and similar magnitude as in our parsimonious specification (column 2 in Table 3), but differences in standard errors arise. While intensity of the intervention remains a strong predictor and low-income clients in low-income economies also benefit

significantly less from financial education, mandatory formats and timing in the sense of offering financial education at a teachable moment appear less predictive of treatment effects.

Interventions in upper-middle and high income economies. Turning to the 72 studies that examine financial education in wealthier economies, we find that results again are qualitatively very similar to the pooled analysis in column 2. Here, the opposing coefficients for mandatory formats and offering financial education at a teachable moment are statistically significant at the 5%-level, indicating that these effects may be primarily driven by interventions in middle or high income economies.

Interventions for low-income individuals. Examining the subsample of 44 studies focusing on low-income individuals results in a similar picture arising. Effects appear to be higher with increased training intensity and offering financial education at a teachable moment. However, country-level years of schooling and country income are now insignificant covariates. Additionally, the coefficient for mandatory courses still has the same sign and magnitude, but is estimated with a larger standard error.

Disaggregating financial behaviors and financial behaviors by target group. As discussed in Section 4.2, it appears to be easier to affect financial behaviors in terms of (retirement-) savings and budgeting compared to borrowing behavior. Thus, we disaggregate the sample into three categories of financial behaviors and search for potentially heterogeneous effects of our main explanatory variables. We reduce the choice of variables for some subsamples to avoid problems with degrees of freedom due to relative few observations.

Column 1 of Table 4 shows results for the subsample with effect sizes on borrowing behavior. This result matches our main results of the aggregated sample of effect sizes with significant positive effects from increased intensity, negative effects for low-income target groups, and countries, negative effects from making financial education mandatory and positive effects from offering financial education at a teachable moment. Column 4 of Table 4 shows results for the subsample of studies that focus on borrowing as the outcome and have low-income clients as the target group. Again, results are nearly identical. However, the delay in measurement is now a marginally significant predictor: Effect sizes in this sample seem to diminish as time between intervention and measurement of outcomes increases. Hence, treatment effects on debt related behaviors among low-income individuals may be shorter-lived.

<Table 4 about here>

Turning to effect sizes on (retirement-) saving (column 2 of Table 4), we observe that the relevant variables from column (1) remain significant predictors. However, voluntary vs. mandatory formats seem to be unrelated to effectiveness. Column 5 of Table 4 only shows the results on savings and retirement savings for low-income individuals. Signs and magnitude are very similar to the benchmark estimation, but the only coefficients estimated with a small standard error are intensity per week and the teachable moment. Thus qualitative results hold, but effect sizes on saving behavior for low-income individuals may be difficult to impact.

Turning to budgeting and record keeping behavior (column 3 of Table 4), on which financial education yields the highest effects, we find that intensity is not significantly related to effect size. Additionally, all of the other signs and relative magnitudes of the coefficients remain similar to our benchmark estimation; however, with increased standard errors due to only 20 studies and 40 observations. Completing this exercise, we now examine determinants of treatment effects for the subsample of budgeting outcomes and low-income clients. There are 11 studies in this subsample reporting 27 estimates. Again, qualitative results are similar and intensity now, again, is a marginally significant predictor of effect sizes on budgeting behavior.

Overall we find that the positive effects from increased intensity appear to be driven more by interventions focused on saving and borrowing behavior, whereas the timing and voluntary participation matter, especially for borrowing behavior. Thus, the financial behavior that is hardest to impact (borrowing) needs special effort in the sense of increased intensity and timing the financial education intervention at a teachable moment.

6 Robustness

The robustness tests cover eight different aspects and are reported in full in Appendix C. All of them confirm our qualitative findings. Here, we just mention these tests: (i) testing the average treatment effect with several alternative meta-regression models; (ii) repeating the parsimonious benchmark model without imputing missing values; (iii) running this model for studies about the U.S. only; (iv) running this benchmark model with classroom studies only; (v) running this model with equal weight per study by either calculating one synthetic effect size per study or weighting

effect sizes accordingly; (vi) running the benchmark specification with different empirical approaches; (vii) analyzing the influence of delay on effects; and (viii) testing a different definition of training intensity. Additionally, we further examine publication selection bias and possible heterogeneity in study-quality in Appendix D.

7 Concluding policy discussion

This meta-analysis covers studies that potentially contribute to realizing policy objectives, such as improved financial literacy and changes in individual financial behavior. Due to this close link to economic policy, we discuss insights that have potential policy relevance in three steps, from general policy lessons over sample-specific lessons to open issues:

General policy lessons. (i) The most important policy lesson from our research is that financial education can be effective, despite several kinds of fundamental criticism. However, the field of financial education is not developed enough that established standards could be followed "blindly," rather the process of designing interventions needs careful attention due to large heterogeneity across program types and individual studies.

(ii) Interventions targeting improvements in financial literacy are quite successful as they achieve effectiveness similar to comparable education interventions in other domains. As financial literacy education basically aims at improving financial knowledge, it seems evidentiary that it works well in the classroom and at school. Improved financial literacy also has an indirect positive effect on financial behavior, although this indirect effect is small so that changes in financial behavior preferably should also be addressed directly.

(iii) Education interventions targeting financial behavior have desired effects on average. Although these effects are economically rather small, they are statistically robust to all kinds of tests including subsamples only covering RCTs. Impacts on financial behavior are higher if the intensity of education is increased and if financial education is offered at a teachable moment. The effects is smaller if 'problematic' groups are addressed, such as low-income clients.

To give an example: Assuming that the financial education program is evaluated by conducting a randomized experiment (RCT=1), treatment effects are reported as intent-to-treat effects (TOT=0), and everything else (delay in measurement, study size, duration and weekly intensity, country income, population literacy, and program

details) is kept at the empirical mean, the predicted effect size on financial behavior would be g=0.043 (SE=0.014, p=0.003) for average income clients, and g=0.024 (SE=0.015, p=0.107) for low-income clients. If financial education is not mandatory and provided at a teachable moment, effectiveness goes up to g=0.085 (SE= 0.020, p=0.000) for average income clients and to g=0.066 (SE=0.021, p=0.002) for low-income clients.

Policy lessons for subsamples. As the universe of studies covers widely diverse financial education interventions regarding the target groups, the intended outcomes, or the intervention channels, it seems advisable to also look at lessons for more homogeneous subsamples. In the following we draw three such lessons. (i) Regarding the country groups, education effects seem to be somewhat lower in *low and lower middle-income countries*. This is probably due to the disadvantageous institutional circumstances in these countries. A relative advantage in these countries, however, is that the general level of education (mean years of schooling in the population) is comparatively low so that marginal returns to additional domain-specific education are high. The lower opportunity costs of education may be a reason why mandatory participation conditions, such as school based programs, are less problematic and offering financial education at teachable moment appears to be of lesser importance in these countries.

(ii) While problematic target groups, such as *low-income clients,* are more difficult to educate in general, the determinants of effective financial education are not different from the general population. If there is a difference, it appears that the timing of interventions in the sense of offering financial education and advice at a teachable moment is relatively important, indicating that there is a particular need to get the attention of this target group.

(iii) Regarding the *outcomes* of financial education, improving debt related behavior is, on average, hardly successful. At the same time, mistakes can be rather consequential and the structure of many significant determinants is the same as for other financial behaviors, such that the general lessons may translate to this specific case; however, it needs much more input to reach economically significant results. Moreover, there is variation across studies revealing clear success cases, which suggests that it is useful to go down to the study level and learn from best practices. The effects on improving savings or budgeting behavior are much larger in magnitude

than on borrowing, while the structure of determinants is also, again, basically unchanged relative to the general case.

Research on open policy issues. In order to improve financial education policies in the future we see three areas of urgent research. (i) We need quite generally *more reliable evidence on the effectiveness* of financial education interventions. Almost two-thirds of the evidence comes from the U.S., indicating that there are large gaps of evaluation elsewhere.

(ii) Regarding the documentation of impact evaluations within published reports, it would be very desirable to provide *more information about study and program characteristics* in general. This is already emphasized by Miller et al. 2015 and we explicitly agree with this suggestion in order to advance the literature. A straight forward example is the quality of teacher training or implementation, which can make a crucial difference but is unknown in almost all studies (Brown et al. 2016). The same applies to the ways in which the curriculum is structured and implemented (see Drexler et al. 2014 as a notable exception).

(iii) Finally, in order to come closer to *welfare assessments* information in two directions is needed: first, information about program costs is frequently missing. Thus, in terms of welfare, positive education effects could be balanced with the true costs of the intervention (see also Lusardi et al. 2016). Second, the discussion of effectiveness of financial education policy should also consider principal alternatives to financial education in general. Such alternatives include limiting the kind of available products (choices), altering the choice architecture (e.g. Caroll et al. 2009), working with nudges (e.g. Thaler and Benartzi 2004; Willis 2011), considering the promotion of commitment devices (e.g. Brune et al. 2016), offering incentives (e.g. Saez 2009), or implementing more rigid consumer financial protection policies (cf. Campbell et al. 2011).

Although our analysis does not provide exact information to facilitate concrete policy decisions, there are two arguments in favor of implementing financial education. First, the small average effect comes with low average intensity. More than 70% of our considered studies invest no more than two days in education, indicating that these measures have not only small effects, but also low costs. Second, the average small effect of financial education is accompanied by large heterogeneity, indicating that those offering financial education measures can still learn from best practice

experiences, a development that is ongoing as evidenced by time trend of slowly increasing effectiveness documented in rigorous impact evaluation studies.

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Table 1: Effect sizes of financial education by intervention type, research design, and country groups

Outcome	Type	Studies	Obs.	ES (g)	SE_g	p-value	Diff. (t-value)
A Effect sizes by intervention channel & setting							
Fin. literacy	Classroom	58	135	0.294	0.054	0.000	0.106**
	Non-classroom	9	55	0.188	0.039	0.001	(2.015)
	- Online	*5*	*41*	*0.217*	*0.060*	*0.018*	
	- Counseling	*0*	-	-	-	-	
	- Nudge	*4*	*14*	*0.103*	*0.045*	*0.108*	
Fin. behavior	Classroom	70	317	0.084	0.013	0.000	-0.014
	Non-classroom	20	32	0.098	0.020	0.000	(0.452)
	- Online	*11*	*18*	*0.085*	*0.034*	*0.031*	
	- Counseling	*7*	*8*	*0.095*	*0.030*	*0.020*	
	- Nudge	*2*	*6*	*0.140*	*0.007*	*0.031*	
Fin. literacy	School	35	62	0.373	0.076	0.000	0.163***
	Non-school	32	128	0.210	0.035	0.000	(3.273)
	- Workplace	*1*	*1*	*0.164*	*0.063*	-	
	- Other	*31*	*127*	*0 210*	*0.035*	*0.000*	
Fin. behavior	School	27	90	0.057	0.014	0.000	-0.039*
	Non-school	63	259	0.096	0.014	0.000	(1.96)
	- Workplace	*17*	*47*	*0.121*	*0.049*	*0.023*	
	- Other	*46*	*212*	*0.090*	*0.015*	*0.000*	
B Effect sizes by research design							
Fin. literacy	RCTs	33	135	0.209	0.033	0.000	-0.185***
	Quasi-exp.	34	56	0.394	0.083	0.000	(-3.638)
Fin. behavior	RCTs	40	220	0.081	0.015	0.000	-0.012
	Quasi-exp.	50	122	0.093	0.022	0.000	(-0.661)
C Effect sizes by country group							
Fin. literacy	High income	47	107	0.328	0.057	0.000	0.183***
	Developing	14	67	0.145	0.031	0.000	(3.787)
	- Low	*3*	*6*	*0.219*	*0.069*	*0.086*	
	- Lower-middle	*6*	*44*	*0.155*	*0.047*	*0.023*	
	- Upper-middle	*5*	*17*	*0.092*	*0.023*	*0.017*	
Fin. behavior	High income	62	140	0.071	0.019	0.000	-0.027
	Developing	20	120	0.098	0.014	0.000	(-1.512)
	- Low	*6*	*39*	*0.161*	*0.038*	*0.009*	
	- Lower-middle	*12*	*84*	*0.091*	*0.008*	*0.000*	
	- Upper-middle	*6*	*52*	*0.06*	*0.023*	*0.045*	

Notes: Average effect sizes *(g)* estimated via OLS regressions of effect sizes fitting only an intercept. Sample is split by an indicator of intervention type, research design or country group. "Channel" is a categorical variable operationalized in the form of four dummy variables: Classroom, Counseling, Online, and "Nudge" where "Nudge" is the default (omitted) category in the regressions. "Setting" is a categorical variable operationalized through three dummy variables: School, Workplace and Other where Other is the omitted category in the meta-regression analyses. Country groups are based on the World Bank Atlas method and refer to 2015 data on GNI per capita. Low-income economies are defined as those with a GNI per capita of $1,025 or less in 2015, lower-middle income economies are defined by a GNI per capita between $1,026 and $4,035, upper-middle income economies are those with a GNI per capita between $4,036 and $12,475, and high income economies are defined by a GNI per capita greater than $12,475. Standard errors are clustered at the study-level. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table 2: Summary statistics at the study level

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
RCT	126	0.405	0.493	0.000	1.000
TOT	115	0.452	0.500	0.000	1.000
Delay	93	82.231	273.613	0.000	1566
1/SE	126	57.535	210.450	2.480	1636.712
Intensity	87	11.211	14.929	0.100	87.000
Duration	76	7.341	14.150	1.000	103.000
Age	109	30.717	14.120	9.000	63.870
Percent female	123	54.011	18.493	0.000	100.000
Low income clients	102	0.529	0.502	0.000	1.000
Years of schooling	126	11.270	2.843	3.200	13.600
FL in population	126	50.340	11.658	24.000	66.000
Mandatory	96	0.292	0.457	0.000	1.000
Incentivized	86	0.314	0.467	0.000	1.000
Teachable moment	126	0.397	0.491	0.000	1.000

Notes: RCT" is a dummy variable with "1" if selection into treatment was conducted through randomization and "0" otherwise (such as matched designs). "TOT" is a dummy variable with "1" if the effect size estimate is derived from the treatment effect on the treated and "0" if it is derived from the ITT estimate. "Delay" is a continuous variable indicating the delay between treatment and measurement of outcomes in weeks. "1/SE" is the inverse standard error for each effect size estimate. "Intensity" is the total number of hours of financial education exposure to the treated. "Duration" indicated the time-frame of financial education in weeks. "Age" is the mean age of the sample in years. "Percent Female" is the relative frequency of female participants in the sample in percent. "Low income" is a dummy variable with "1" if the mean annual income per capita of the sample is below the country average income per capita. "Mandatory" is a dummy variable with "1" indicating mandatory participation in financial education and "0" voluntary participation. "Incentivized" is a dummy variable with "1" when incentives to participate where provided and "0" if participation was unconditional on incentives. "Teachable moment" is a dummy variable indicating whether the financial education intervention was offered at a teachable moment.

Table 3: Explaining heterogeneity in effect sizes on financial behavior

	(1) All	(2) All	(3) RCTs	(4) Low inc. econ	(5) High / middle inc. econ	(6) Low income clients
RCT	-0.070**	-0.068**		-0.209**	-0.079**	-0.066**
	(0.027)	(0.028)		(0.091)	(0.036)	(0.032)
TOT	0.079***	0.068**	0.012	-0.016	0.076**	0.031
	(0.027)	(0.027)	(0.040)	(0.066)	(0.035)	(0.032)
Delay	0.000	0.000	-0.001**	-0.001**	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1/SE	-0.000	-0.000	0.000	-0.003	-0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.002)	(0.000)	(0.000)
Intensity / week	0.004**	0.004***	0.007***	0.004**	0.003	0.004***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)
Duration	-0.000	-0.000	-0.001	-0.001	-0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
Age	-0.001					
	(0.001)					
Percent female	-0.000					
	(0.001)					
Low income clients	-0.065***	-0.055***	-0.074***	-0.042**	-0.048**	
	(0.020)	(0.017)	(0.024)	(0.019)	(0.021)	
Years of schooling	-0.016***	-0.019***	-0.016**	-0.026***	-0.025***	-0.011*
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.006)
FL in population	-0.003					
	(0.002)					
Country group						
a.) Low/lower-mid. inc. econ.	-0.129*	-0.093**	-0.092**			-0.059
	(0.073)	(0.036)	(0.042)			(0.042)
b.) Upper-mid. inc. econ.	0.000					
	(0.060)					
Channel						
a.) Classroom	-0.003					
	(0.028)					
b.) Counseling	-0.018					
	(0.033)					
c.) Online	-0.028					
	(0.028)					
Setting						
a.) School	0.022					
	(0.023)					
b.) Workplace	0.041					
	(0.036)					
Mandatory	-0.074***	-0.051**	-0.078*	-0.015	-0.065**	-0.052
	(0.024)	(0.023)	(0.044)	(0.042)	(0.025)	(0.033)
Incentivized	-0.012					
	(0.029)					
Teachable moment	0.079***	0.064**	0.016	0.025	0.069**	0.072**
	(0.021)	(0.026)	(0.035)	(0.026)	(0.029)	(0.032)
Constant	0.477***	0.332***	0.338***	0.514***	0.406***	0.188*
	(0.157)	(0.079)	(0.095)	(0.110)	(0.114)	(0.095)
R^2	0.210	0.183	0.149	0.170	0.204	0.109
n (Studies)	90	90	40	18	72	44
n (Effect sizes)	349	349	227	129	220	234

Notes: Non-standardized coefficients from OLS regressions. Dependent variable in columns (1) and (2) is effect size (Hedges' g) on financial behavior in the full sample of studies reporting on financial behavior as an outcome. Column (3) shows results for RCTs only. Column (4) and (5) show results for financial behavior split by country groups. Column (6) limits the sample to classroom trainings only. Robust standard errors clustered at the study-level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table 4: Heterogeneity in effect sizes for subsamples by type of financial behavior and target group

	(1) Borrow	(2) Save	(3) Budget	(4) Borrow × low inc. clients	(5) Save × low inc. clients	(6) Budget × low inc. clients
RCT	-0.136***	-0.010		-0.100***	-0.035	
	(0.022)	(0.044)		(0.026)	(0.058)	
TOT	0.089**	0.090*		0.106**	0.074	
	(0.033)	(0.051)		(0.039)	(0.079)	
Delay	-0.000	0.000	-0.001	-0.000*	-0.000	-0.019
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.012)
1/SE	0.000	-0.000	-0.003*	0.001**	0.000	-0.007
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.005)
Intensity / week	0.003**	0.003**	0.037	0.003**	0.004**	0.595*
	(0.001)	(0.001)	(0.031)	(0.001)	(0.002)	(0.308)
Duration	-0.000	-0.001	-0.000	-0.000	0.000	0.017
	(0.000)	(0.001)	(0.003)	(0.000)	(0.001)	(0.014)
Low income clients	-0.043**	-0.055***				
	(0.019)	(0.021)				
Years of schooling	-0.023***	-0.021***	-0.020*	-0.023***	-0.011	0.017
	(0.006)	(0.006)	(0.011)	(0.008)	(0.011)	(0.022)
Low/lower-mid. inc. econ.	-0.178***	-0.154***		-0.199***	-0.102	
	(0.052)	(0.046)		(0.067)	(0.066)	
Mandatory	-0.069**	-0.022		-0.120***	-0.010	
	(0.032)	(0.029)		(0.039)	(0.049)	
Teachable moment	0.100***	0.082**		0.087***	0.114*	
	(0.025)	(0.037)		(0.026)	(0.065)	
Constant	0.375***	0.337***	0.361**	0.326**	0.147	-0.685
	(0.087)	(0.090)	(0.134)	(0.114)	(0.165)	(0.524)
R^2	0.473	0.200	0.206	0.394	0.147	0.359
n (Studies)	32	71	20	20	31	11
n (Effect sizes)	100	177	40	73	91	27

Notes: Non-standardized coefficients from OLS regressions with clustered standard errors at the study-level in parentheses. We only include right hand side variables where differential information from at least two studies is available in the regressions. ***, ** and * denote significance at the 1%, 5% and 10% level.

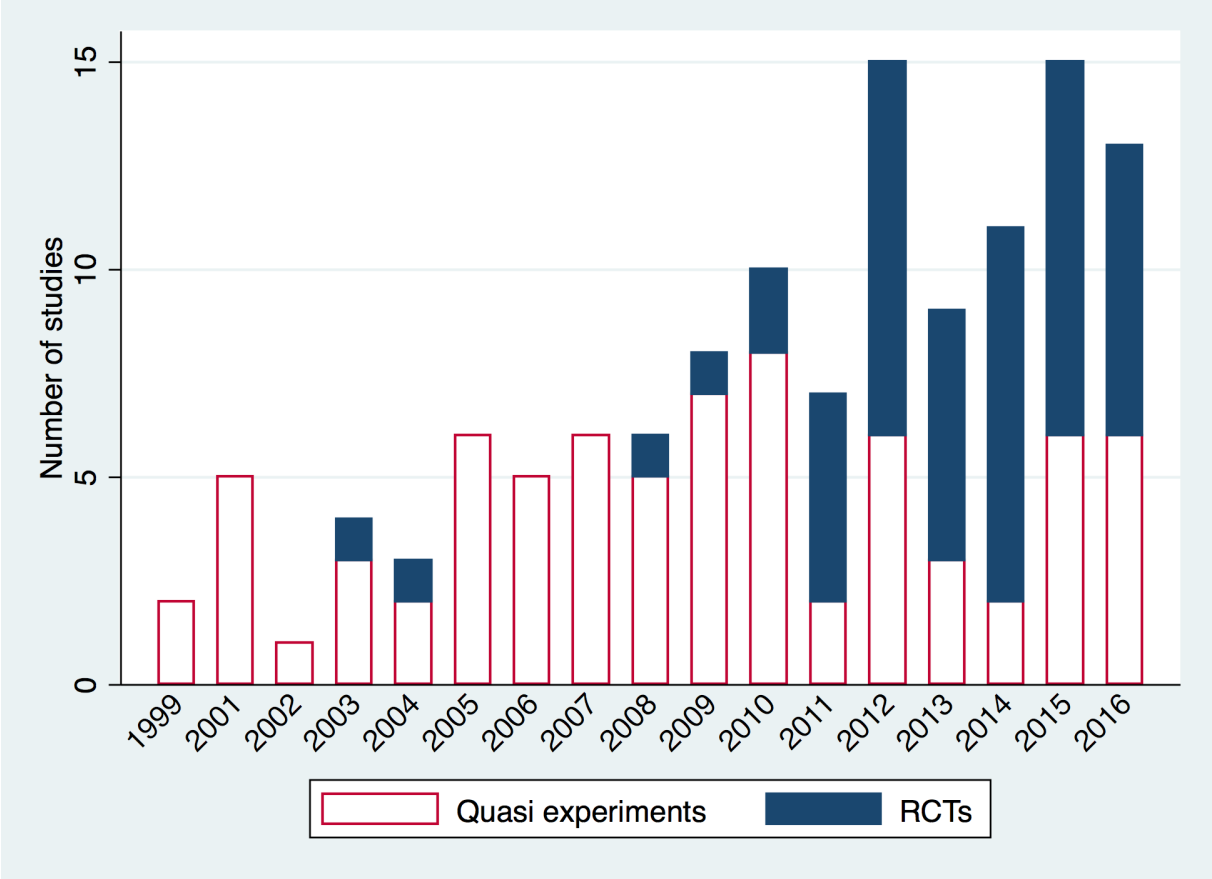

Figure 1: Number of studies in our sample by research design per year

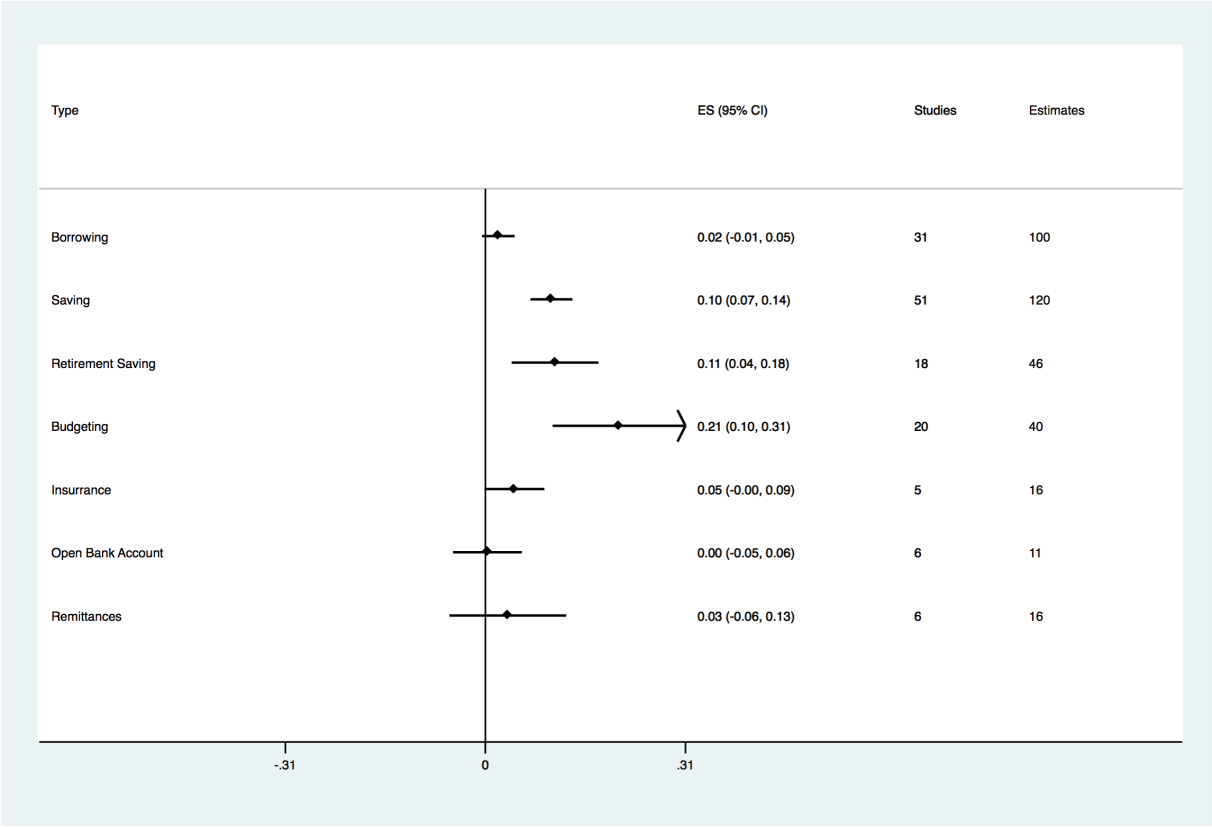

Figure 2: Forest plot of effect sizes by type of financial behavior studied

Appendix

(online appendix not intended for publication)

to accompany

"Does financial education impact financial behavior, and if so, when?"

Appendix A: Supplementary material

This Appendix A contains two kinds of information: First, there are three tables (Table A1 to Table A3) and two figures (Figure A1 and Figure A2), which are referred to in the main text, mainly in the earlier sections.

Second, there is a longer documentation about "Additional information on selection of studies and extraction of effect size estimates and study descriptors." This documentation provides deeper information that complements Section 3.1 (Selection of studies) and Section 3.2 (Extraction of effect size estimates and study descriptors) of the main text.

Table A1: Summary of financial education studies by publication date and country

	Number of studies (1)	Percent of sample (2)	
A By publication date			
1999	2	1.59	
2000	0	0.00	
2001	5	3.97	
2002	1	0.79	
2003	4	3.17	
2004	3	2.38	
2005	6	4.76	
2006	5	3.97	
2007	6	4.76	
2008	6	4.76	
2009	8	6.35	
2010	10	7.94	
2011	7	5.56	
2012	15	11.9	
2013	9	7.14	
2014	11	8.73	
2015	15	11.9	
2016	13	10.32	
B By country of intervention			*Income*
Australia	2	1.59	High
Bosnia and Herzegovina	1	0.79	Upper-middle
Brazil	1	0.79	Upper-middle
China	1	0.79	Upper-middle
Dominican Republic	1	0.79	Upper-middle
Germany	1	0.79	High
Ghana	1	0.79	Lower-middle
Hong Kong, China	1	0.79	High
India	8	6.35	Lower-middle
Indonesia	2	1.59	Lower-middle
Italy	7	5.56	High
Kenya	1	0.79	Lower-middle
Malawi	1	0.79	Low
Mexico	1	0.79	Upper-middle
Mozambique	1	0.79	Low
New Zealand	2	1.59	High
Pakistan	1	0.79	Lower-middle
Qatar	1	0.79	High
Rwanda	1	0.79	Low
Singapore	1	0.79	High
South Africa	1	0.79	Upper-middle
Spain	1	0.79	High
Sri Lanka	1	0.79	Lower-middle
Tanzania	2	1.59	Low
USA	83	65.87	High
Uganda	2	1.59	Low
Low inc. econ.	7	5.5	
Lower-middle inc. econ.	14	11.11	
Upper-middle inc. econ.	6	4.76	
High inc. econ.	99	78.57	
Total	126	100	

Notes: Country group classifications refer to 2015 World Bank data on GNI per capita (Atlas method).

Table A2: Overview of coded outcomes and definitions

	Outcome category	Definition	Freq.
Financial literacy (190 estimates)			
A	*Financial knowledge (+)*	Raw score on financial knowledge test Indicator of scoring above a defined threshold Indicator of solving an item correctly	190 (100%)
Financial behaviors (349 estimates)			
B	*Borrowing & debt management behavior*		100 (28.65%)
	1) Reduction of loan default within a certain time-frame (+)	Binary indicator	
	2) Reduction of delinquencies within certain time frame (+)	Binary indicator	
	3) Better credit score (+)	Continuous measure of credit score	
	4) Reduction in informal borrowings (+)	Binary indicator of informal loan or reduction in number of informal loans	
	5) Lower cost of credit / interest rate (+)	Sum of real interest amount or interest rate and (if applicable) cost of fees	
	6) Any debt (-) / (+) (depending on intervention goal)	Binary indicator Binary indicator	
	7) Any formal loan (+)	Continuous measure of borrowed amount	
	8) Total amount borrowed (-) / (+) (depending on intervention goal)		
	9) Total outstanding debt (-) / (+) (depending on intervention goal)	Continuous measure of total debt	
	10) Better borrowing index (+)	Study-specific index of survey items to measure borrowing amount, frequency, and repayment	
	11) Uses credit card up to limit (-)	Binary indicator	
C	*Budgeting & planning behavior*		40 (11.46%)
	1) Having a written budget (+)	Binary indicator	
	2) Positive sentiment toward budgeting (+)	Binary indicator	
	3) Having a financial plan (+)	Binary indicator	
	4) Keeping separate records for business and household (+)	Binary indicator	
	5) Seeking information before making financial decisions (+)	Binary indicator	
	6) Self-rating of adherence to budget (+)	Study-specific scale	
D	*Saving & retirement saving behavior*		166 (47.56%)
	1) Total savings held (+)	Continuous measure of savings amount or categorical variable indicating amount within range	
	2) Savings rate or savings within timeframe (+)	Savings relative to income Amount over defined time-frame	
	3) Savings index (+)	Study-specific index of survey items designed to measure savings amount and frequency	*-continued-*
	4) Any savings (+)	Binary indicator	

	5) Has formal bank (savings) account (+)	Binary indicator	
	6) Investments into own or other business (stocks) (+)	Continuous measure of amount invested	
	7) Holds any stocks or bonds (+)	Binary indicator	
	8) Has any retirement savings (+)	Binary indicator	
	9) Participates in retirement savings plan (e.g. 401k) (+)	Binary indicator	
	10) Amount of retirement savings (+)	Continuous measure of retirement savings amount	
	11) Retirement savings rate (+)	Retirement savings relative to income	
	12) Positive sentiment towards investing funds (+)	Binary indicator	
	13) Reduction of excess risk in retirement fund (+)	Continuous measure of retirement savings amount allocated to risky assets	
	14) Reduction of cost of savings product (fees paid) (+)	Continuous measure of fee amount paid	
	15) Increase in contribution rate to retirement savings plan (+)	Indicator of increase or continuous measure of amount increase	
	16) Net wealth (+)	Continuous measure of net wealth	
E	*Insurance & risk mitigation behavior*		16 (4.59%)
	1) Any formal insurance (+)	Binary indicator	
	2) Having a diversified portfolio (+)	Numbers of assets in portfolio; Standard deviation of returns in portfolio	
F	*Remittance behavior*		16 (4.59%)
	1) Lower cost of remittance product (+)	Continuous measure of cost or binary choice of lower cost product	
	2) Lower remittance frequency and higher amount (lower cost) (+)	Measure of remittance frequency within timeframe and continuous amount remitted	
	3) More control over remitted funds (+)	Study-specific scale to measure control over remitted amount	
G	*Bank account behavior*		11 (3.15%)
	1) Has formal bank account (+)	Binary indicator	
	2) Opens formal account within certain time frame	Binary indicator	
	3) Uses formal bank account	Binary indicator	

Notes: When necessary, outcomes are reverse-coded so that positive signs reflect positive financial education treatment effects (i.e. when the dependent variable is coded as the probability of default, we transform this to the reduction in probability of default in order to be able to assign a positive sign).

Table A3: Summary of estimated financial education impacts

Outcome	Significance at 5%			Significance at 10%			Average effect size
	Negative	*Insig.*	*Positive*	*Negative*	*Insig.*	*Positive*	(SE)
A Effects on financial literacy							
Fin. literacy	1 (0.53%)	72 (37.89%)	117 (61.58%)	2 (1.05%)	62 (32.63%)	126 (66.32)	0.263*** (0.414)
B Effects on financial behavior							
Fin. behavior	8 (2.29%)	215 (61.60%)	126 (36.10%)	18 (5.16%)	181 (51.86%)	150 (42.98%)	0.086*** (0.012)
Borrowing	5 (5.00%)	80 (80.00%)	15 (15.00%)	10 (10.00%)	70 (70.00%)	20 (20.00%)	0.023 (0.014)
Budgeting & planning	0 (0.00%)	15 (37.5%)	25 (62.50%)	1 (2.50%)	10 (25.00%)	29 (72.50%)	0.207*** (0.053)
Saving	2 (1.67%)	61 (50.83%)	57 (47.50%)	6 (5.00%)	49 (40.83%)	65 (54.17%)	0.108*** (0.017)
Retirement Saving	0 (0.00%)	22 (47.83%)	24 (52.17%)	0 (0.00%)	17 (36.96%)	29 (63.04%)	0.108*** (0.034)
Insurance	0 (0.00%)	13 (81.25%)	3 (18.75%)	0 (0.00%)	12 (75.00%)	4 (25.00%)	0.045 (0.024)
Bank account behavior	0 (0.00%)	10 (90.91%)	1 (9.09%)	0 (0.00%)	10 (90.91%)	1 (9.09%)	0.003 (0.027)
Remittance behavior	1 (6.25%)	14 (87.50%)	1 (6.25%)	1 (6.25%)	13 (81.25%)	2 (12.50%)	0.035 (0.046)

Notes: Average effect sizes are estimated via OLS with standard errors clustered at the study-level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

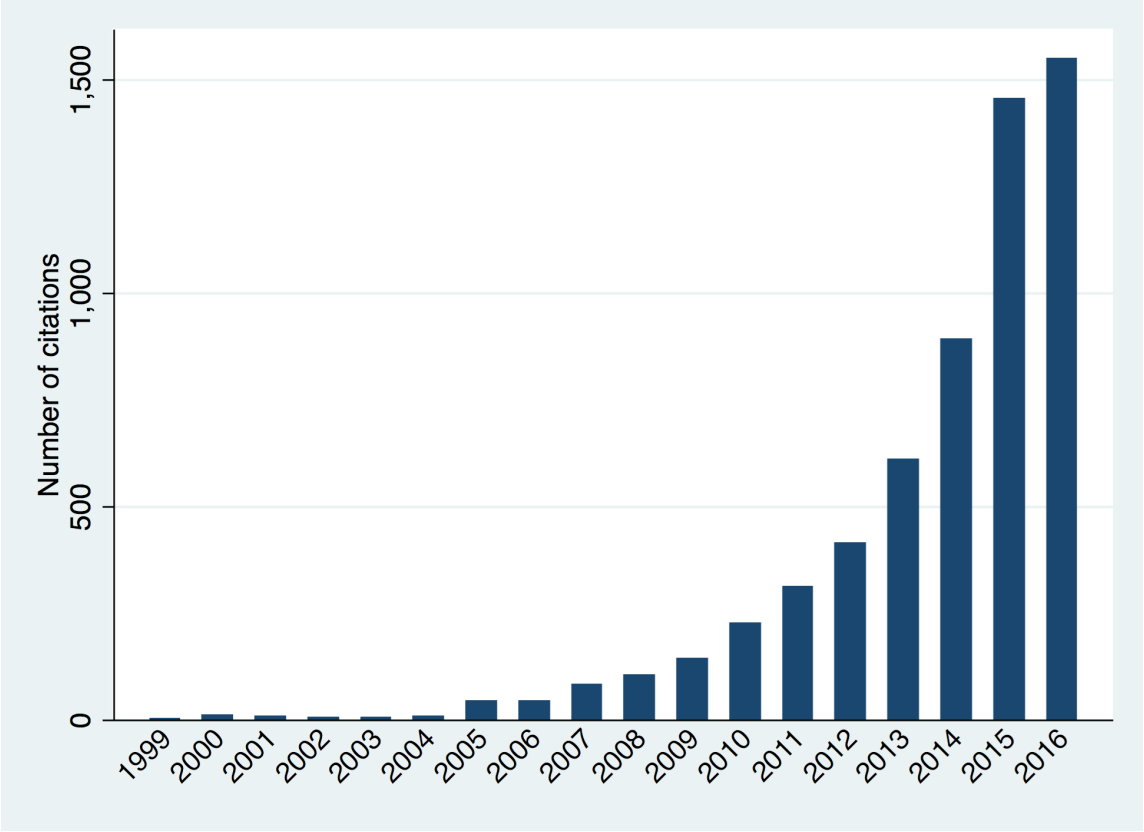

Figure A1: Citations of published items with the keyword *financial literacy* per year, source: SSCI

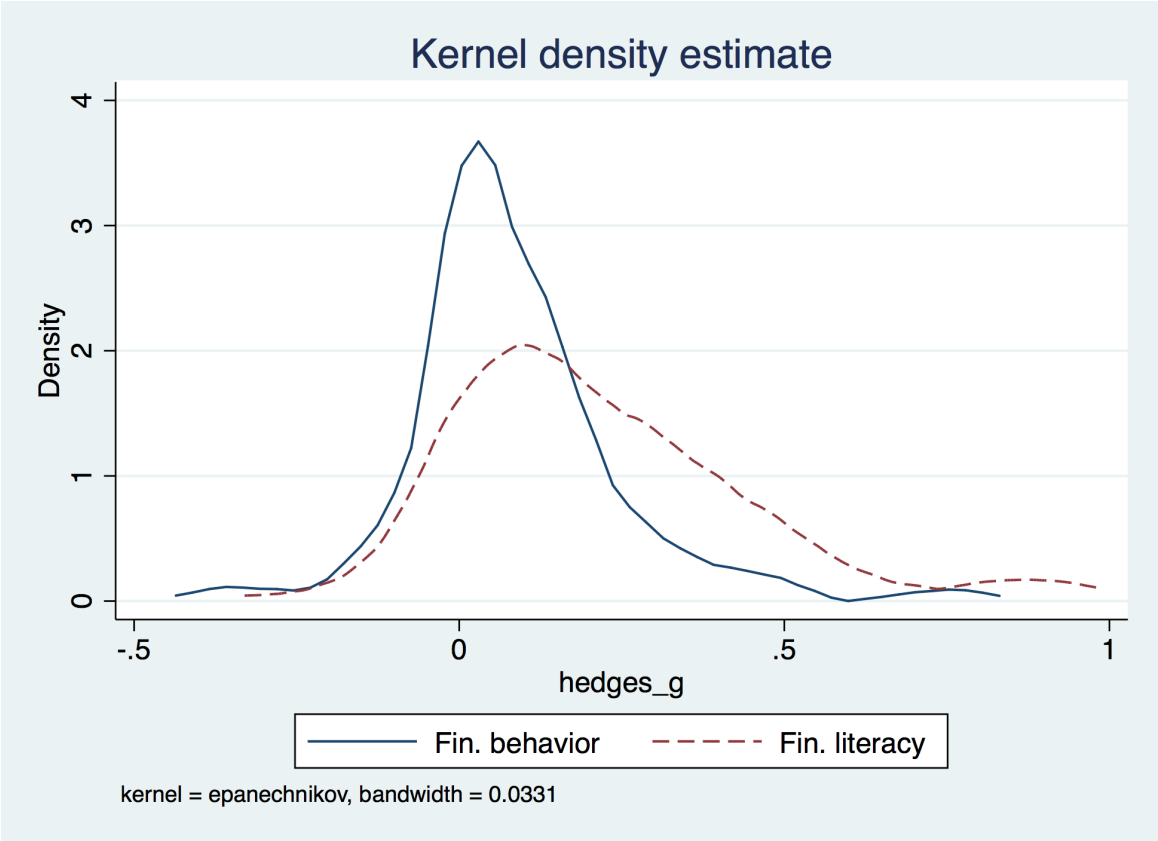

Figure A2: Kernel-density estimates of effect sizes by outcome (for Hedge's g<1

Additional information on selection of studies and extraction of effect sizes estimates and study descriptors.

Selection of studies. We follow the established meta-analytical protocol (cf. Lipsey and Wilson 2001, p.23; Stanley 2001, p.143; Stanley and Doucouliagos 2012; Stanley et al. 2013). This starts with systematically searching the relevant databases for the most common keywords in order to aggregate a large sample of potentially eligible studies to be included in our meta-analysis. Keywords are (i) financial literacy; (ii) financial knowledge; (iii) financial education; (iv) financial capability; and (v) combinations of these keywords with "intervention." To minimize publication bias and capture the broadest sample of studies possible, we systematically search not only the relevant databases for published records (e.g. ISI, Business Source Premier via EBSCO Host, JStor) but also for registered trials, working papers, and informal research reports (e.g. AEA RCT-registry, SSRN, Fin. Lit. E-Journal, RePEC, NBER, Worldbank eLibrary). All records from recent systematic accounts of the literature (Fernandes et al. 2014; Miller et al. 2015) are included in our initial pool of studies. In addition, we screen the references of narrative literature reviews (Fox et al. 2005; Collins and O'Rourke 2010; Willis 2011; Xu and Zia 2012; Hastings et al. 2013; Blue et al. 2014; Lusardi and Mitchell 2014).

This search resulted in over 500 potentially relevant published journal-articles and over 600 results from working paper databases with some apparent overlap. We stopped collecting articles from these databases in October 2016.

From this collection, we drop studies that do not meet our three criteria of inclusion: (i) Reporting on impacts of an exogenous educational intervention designed to strengthen the participants' financial literacy and/or leading to behavioral change in the area of personal finance; (ii) providing a quantitative assessment of intervention impact that allows coding an effect size statistic (g) and its standard error; and (iii) relying on an observed counterfactual in the estimation of intervention impacts. Consequently, we only include experimental studies with sufficient information on intervention outcomes in our analysis, i.e. RCTs, quasi-experiments, and natural experiments (see below for coding of studies). Where necessary information was partially missing, we consulted additional online resources related to the article or contacted the authors of the primary studies via e-mail.

This selection-process results in a final sample of 126 independent intervention studies that report 539 effect sizes. Of these, 90 studies report 349 effect sizes on financial behavior, and 67 studies report 190 effect sizes on financial literacy. Among these 90 plus 67 studies, there are 31 studies reporting effect sizes for both financial literacy and behavior. Our selection of studies covers 126 independent interventions from 1999 through 2016. Table A1 shows the composition of our sample of studies by the date of publication (*Panel A*) and the country in which the intervention took place (*Panel B*). While most interventions took place in the U.S. and other OECD countries, 21.4% of studies were conducted in low- or middle-income countries. The sample is comprised of 51 RCTs and 75 quasi-experiments. RCTs are rare in the early years of the literature, but the share has risen dramatically, with the majority of studies conducted from 2011 onward being randomized evaluations (see Figure 1).

Extraction of estimates. The next step in our meta-analytic process is to extract effect size estimates from the statistical data reported in the primary studies. Our analysis aggregates treatment effects of financial education interventions on two main categories of outcomes. First, we code the effect of financial education on *financial literacy* (i.e. a measure of performance on a financial knowledge test) since knowledge development is the primary goal of financial education (Hastings et al. 2013; Lusardi and Mitchell 2014). We do not include self-assessments of changes in financial knowledge as an outcome.

Second, we code treatment effects of financial education on *financial behaviors.* These behaviors can be further disaggregated into the following categories: Borrowing, savings and retirement saving, budgeting and planning, insurance, as well as remittances. Table A2 provides an overview of the categories and definitions of effect size estimates by outcome type.

We code all available effect sizes per study on cognitive (financial knowledge) and behavioral outcomes. We include multiple estimates per study if multiple outcomes, time-points, or treatments are reported. We only extract main (average treatment) effects reported in the papers. Thus, we do not code estimates reported in the "heterogeneity-of-treatment-effects-section" within papers, such as sample splits or interaction-effects of binary indicators (e.g. gender, income, ability, …) with the treatment indicators. If results are only reported in a disaggregated manner (only effects on subsamples), we perform a within study (random-effects)

meta-analysis (DerSimonian and Laird 1986) to generate an inverse-variance-weighted average effect size to proxy the main effect. Additionally, we aim to capture only non-redundant effect sizes per paper (i.e. we do not include effect sizes for the same intervention on the same outcome reported in the robustness-section). The number of coded estimates per study ranges from 1 to 87 estimates. We show in the Appendix C (robustness checks) that giving each study equal weight by creating a single synthetic effect size per study through a within-study meta-analysis or alternatively by weighting each observation by the inverse number of effect-size estimates contributed by each study yields similar results.

In addition to the coding of all possible estimates of effect sizes (g) and their standard errors of financial education treatment on financial literacy or financial behavior (cf. Section 2), we develop a coding protocol to extract potentially relevant information about the study (study descriptors) that may serve as predictor variables explaining the variability in effect sizes. Specifically, we aim at extracting data on (i) research design and measurement of dependent variables; (ii) the intensity of education; (iii) the sample/target group of the intervention; and (iv) the details of the intervention itself, such as channel, setting, and participation conditions. Coding of the included study reports was completed by the authors of this paper and two research assistants who were trained using the guidelines by Lipsey and Wilson (2001, p.88). Overall intercoder reliability is high and data collection for most of the variables concerning the setting, participants, and research design of the primary studies was straightforward. However, key details of the underlying educational intervention are often missing or underreported in the research reports. If information is only partially missing authors were asked to provide these details via e-mail.

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Appendix B: Comparison of our dataset and results to previous meta-analyses

There are two earlier meta-analyses about financial education: The study by Miller et al. (2015) covers 19 papers due to its extremely restrictive selection criteria. Thus, most similar to our work is the study by Fernandes et al. (2014), which covers 90 effect sizes from financial education reported in 77 papers. Despite an overlap of 44% with their sample of studies, our research differs in four ways which explains our new results: (i) most important is that we analyze determinants of program effectiveness in a broader way by applying respective coding. (ii) Then we code the various outcomes per study and their respective effectiveness. Moreover, (iii) we cover recent and mostly randomized experiments providing evidence of effective interventions; and (iv) we cover additional studies focusing exclusively on financial literacy as the outcome variable. We aim to elaborate on these comparisons in this part of the Appendix.

Comparison of studied samples. Our selection-process (see Appendix A) led us to a final sample of 126 independent intervention studies that report 539 effect sizes. Of these, 90 studies report 349 effect sizes on financial behavior, and 67 studies report 190 effect sizes on financial literacy. Among these 90 plus 67 studies, there are 31 studies reporting effect sizes on both financial literacy and behavior. The sample is comprised of 51 RCTs and 75 quasi-experiments.

As mentioned, Miller et al. (2015) select 19 intervention-studies for their statistical meta-analysis. Their main inclusion criterion is that interventions report on identical outcomes. This limits their analysis to sample sizes of four to six studies (and estimates) per outcome. While informative of magnitude and significance of effect sizes on identical outcomes, such an approach prevents a detailed investigation into the sources of heterogeneity, given the very limited number of studies available. However, we note that the results for size, direction, and significance of the main behaviors studied in Miller et al. 2015 are in line with our results (see Figure 2).

Fernandes et al. (2014), with 77 papers selected, cover 90 effect sizes (15 RCTs and 75 quasi-experiments) of "manipulated financial literacy" (cf. Fernandes et al. 2014, p.1863). Of their 77 papers, 55 are also part of our sample. We exclude 22 single-group pre-posttest and quasi-experimental papers because they either do not analyze education interventions (but other personal finance related programs, e.g.

match incentives), report only aggregate measures of self-reported financial behavior, wellbeing or self-efficacy, or because it is not feasible to calculate a meaningful effect size statistic. In addition, we include 35 recent studies that were not previously available. Moreover, we consider another 36 studies examining the impact of financial education on financial literacy but neglecting possible impacts on financial behavior. These differences explain the mentioned overlap of 44% regarding studies.

Comparison of estimation results. We estimate the average treatment effect of educational interventions on *financial behaviors* to be statistically highly significant (g=0.086, p=0.000, n=349). Although the average treatment effect of 0.086 is small in magnitude, there exists a measurable and robust impact of financial education on various kinds of financial behavior. In comparison, Fernandes et al. (2014) estimate the summary effect of financial education on financial behavior to be roughly g=0.066. However, the authors use averaged effect sizes per paper and weight each observation with its average inverse variance. In order to obtain a better comparison with that study, we exactly apply their method (random effects meta-regression) with synthetic effect sizes per study to our sample of studies. This provides an average (weighted) effect size of g=0.079 (p=0.000, n=90) (see Table C1 in Appendix C). Thus, our estimate of a summary effect for the literature is not too different from theirs.

To investigate the potential source of this difference, we estimate the weighted average effect size among those recent studies that are not included in Fernandes et al. (2014). Indeed, we find that there is a larger average effect of financial education on financial behavior in this sample (g=0.13). This indicates that the new studies covered in our meta-analysis are the main source of difference. Diving deeper into this issue, we find that Fernandes et al. (2014) estimate extremely small average effect sizes for their sample of 15 RCTs. Our broader sample of randomized experiments, however, leads to a much more positive assessment. In line with this observation, the effect size of financial education on financial behavior documented in RCTs seems to increase over time, indicating a positive time trend in effect sizes: a regression of effect size on year of study publication results in a statistically highly significant coefficient (b=0.014, SE=0.004). This moderate, positive time-trend is an important element in explaining our positive result about the effect of financial education on financial behavior.

Turning to the result concerning the treatment effect of financial education on *financial literacy* (measured through knowledge assessments), we estimate the average impact of financial education on financial literacy to be g=0.263 (p=0.000, n=190). Thus, our analysis of a comprehensive sample of studies (n=67) leads to a positive assessment of the effectiveness of financial education on financial literacy. This education explains 1.7% of the variance in financial knowledge and, thus, appears only slightly less effective than educational interventions in other domains, such as math and science instruction (cf. Fernandes et al. 2014, p.1867). Our positive result is in remarkable contrast to Fernandes et al. (2014, p.1867), who find that financial education only explains 0.4% of the variance in financial literacy and state accordingly that, "financial education yields surprisingly weak changes in financial knowledge presumed to cause financial behavior." However, this result seems a bit fragile as it is based on only 12 studies and cannot, obviously, be replicated in our larger sample of studies (cf. Fernandes et al. 2014, p. 1867).

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Appendix C: Robustness checks

Appendix C contains eight kinds of robustness checks: (i) we estimate the (weighted) average treatment effect of financial education on financial behavior using five alternative meta-regression models for continuous effect sizes; (ii) we show results without imputing missing values; (iii) we run our benchmark analysis with the subsample of studies conducted in the USA only; (iv) we run our benchmark analysis with the subsample of classroom financial education studies only; (v) we give each study the same weight in the analysis by creating one synthetic effect size per study or, alternatively, assigning a weight of the inverse number of observations contributed by each study to each estimate within a given study; (vi) we re-estimate our multivariate analysis using eleven alternative meta-regression models; (vii) we look for heterogeneous impacts depending on the delay in measurement of outcomes; and, lastly, (viii) we test a different operationalization of training intensity.

(i) Summary of treatment effects on financial behavior under various models. Table C1 shows the estimated (weighted) average effect size of financial education treatment on financial behavior outcomes for six alternative models. We first perform an analysis on the full sample (Panel A) and disaggregate our sample further into RCTs only (Panel B) and a subsample containing only quasi and natural experiments (Panel C).

<Table C1 about here>

Column (1) repeats the OLS results, while Column (2) shows results with a single synthetic (weighted average) effect size per study. Column (3) shows results for random effects meta-regression (DerSimonian and Laird 1986) with inverse variance weights, synthetic effect sizes per study, and Knapp and Hartung (2003) adjusted standard errors. This is common in meta-analyses in other disciplines (such as clinical trials) and thus serves as a further check of the sensitivity of our results to the estimation strategy. This approach assigns weights for each study based on the inverse variance of the within study measurement error plus the between study variance (tau squared) ($w_i = \frac{1}{V_{Yi}+\tau^2}$). Thus we define our meta-analytic model as

$$g_i = x_i\beta + u_i + \epsilon_i \quad (6)$$

where

$$u_i \sim N(0,\ \tau^2) \tag{7}$$

and

$$\epsilon_i \sim N(0, \sigma_i^2) \tag{8}$$

Here g_i is defined as the effect size estimate of study *i*, $\sigma_i$is the corresponding standard error, $\tau^2$is the between study variance in true effects, and $x_i\beta$ is a vector of study level covariates (including an intercept). We estimate this model using either method of moments (DerSimonian and Laird 1986) or alternatively restricted maximum likelihood.

Column (4) reports estimations based on a GLS random-effects model. If one assumes that the between-study heterogeneity cannot readily be explained by the observable characteristics included, $x_{ij}\beta$ (i.e. due to unobserved heterogeneity in implementation quality), one has to incorporate unobservable characteristics through random effects into the model (cf. Cho and Honorati 2014). Thus, including an effect capturing unobservable characteristics of the study, the meta-analytic model is defined as:

$$g_{ij} = x_{ij}\beta + \theta_{ij} + \epsilon_{ij} \tag{5}$$

where g_{ij} is the impact (continuous effect size) of a financial education intervention on outcome *i* reported in study *j*, $x_{ij}\beta$ is a vector of observable covariates, θ_{ij} is a random effect of unobservable study characteristics and $\epsilon_{ij}$is an error term independent of $x_{ij}\beta$ and θ_{ij}.

Column (5) shows results for full pooling unrestricted weighted least squares using the inverse standard error (precision) as weights (cf. Stanley and Doucouliagos 2012, 2015). Finally, Column (6) shows results from robust variance meta-regression with dependent effect sizes (see Tanner-Smith and Tipton 2014).

Reassuringly the direction is positive and statistical significance is found for all of the considered models and sample splits. Additionally, the magnitude of the coefficient is similar; however differences in detail do exist: The most common meta-analysis model is presented in Column (3), which is also the model that Fernandes et al. (2014) and Miller et al. (2015) use for their analyses. These models

compare favorably to our main results discussed in the paper relying on unrestricted ordinary least squares using multiple estimates per study and clustering the standard errors at the study-level. In contrast, unweighted random effects GLS leads to a higher estimate of the average treatment effect (Column 4). This approach is used previously by Cho and Honorati (2014). The smallest estimate is reported in Column (5): By relying on unrestricted weighted least squares, very large studies with extremely small standard errors, which are most often quasi-experimental, receive extreme weight in the calculation of the summary effect. From our point of view, it does not seem ideal to discount comparatively smaller studies (which often still have sample sizes of over 1000 individuals) with high internal validity (RCTs) as strongly as this approach does. Thus, if one incorporates weights based on the standard error or variance of estimates, it seems advisable to account for between study heterogeneity through random effects as discussed above and presented in columns (3) and (6). Finally, column (6) presents results applying a recently developed method that accounts for dependency among effect sizes (multiple, correlated estimates per study) (see Hedges et al. 2010; Tanner-Smith and Tipton 2014). Again, results are in line with our main results, although with deflated expectations about the average effect in the whole sample of studies. This estimate is also in line with the magnitude of the result presented in Fernandes et al. (2014), however, our assessment about the effectiveness of 40 RCTs on financial behavior remains to be strikingly different to the evidence synthesized by Fernandes et al. (2014).

(ii) Conservative handling of missing data. Next, we turn to estimations of complete cases only, in order to test the robustness of our results using imputed default categories or mean values for missing observations. Column (1) in Table C2 reports OLS meta-regression results for complete cases only. These results correspond to the results presented in Table 3 of the manuscript but show larger standard errors for some of the variables, however, turning none of the main explanatory variables insignificant. This result strongly supports the conclusions drawn from estimations with a large number of studies in the sample.

<Table C2 about here>

(iii) US only subsample. Then we consider only studies conducted in the U.S., since these account for 65.87% of the studies and 42.67% of the effect size

estimates in our sample (column 2 of Table C2). Again, our results are near identical to the estimation in Table 3. However, the standard error for the covariate for low-income clients increases and turns this result insignificant while maintaining its magnitude and sign.

(iv) Classroom trainings only. Further, we consider only studies reporting on classroom trainings as interventions (column 3). Again, our results are near identical to the estimation in Table 3. However, the standard error for the covariate for mandatory courses increases.

(v) Equal study weights. Much of the meta-analysis literature in other fields than economics uses effect size models where each study contributes only one synthetic effect size to the meta-regression analysis. This procedure assures that the assumption of independent estimates is not violated. There are different options to provide such a single effect. Some suggest only using the most robust results in a primary study (cf. Cho and Honorati 2014, p. 119). The textbook literature on meta-analysis, however, tends to recommend creating a synthetic effect size per study by using the average (or weighted average) effect across multiple outcomes (cf. Lipsey and Wilson 2001).

We follow this approach here for the purpose of robustness exercises, but we point at the major disadvantage that effects heading in opposite directions within one study may be cancelling each other out. Column (4) of Table C2 shows results for such an approach. The signs and magnitudes of our coefficients are very similar to the model with multiple non-synthetic effect sizes per study and standard errors clustered at the study-level. However, in the estimation based on this sample, the standard errors increase, thus leading to insignificant covariates in three cases: RCT, intensity per week, and low-income clients. Since this approach works with much less information than would be otherwise available, we conclude that qualitatively this check also confirms our main findings derived from the larger sample of available effect sizes.

Finally, in column (5) we give each study equal weight by assigning the inverse number of estimates per study as weights for each effect size observation within a study. This yields very similar results to the approach in column (4).

(vi) Alternative meta-regression models. Here we discuss the use of alternative statistical regression models in the estimation of predictors of intervention impact.

(Ordered) probit models for sign and significance. In column (1) of Table C3 we apply a probit-regression on an indicator variable of statistically significant effect estimates (at the 5%-level). This is a departure from earlier analyses because we now neglect the size of effects and only consider their statistical significance. Following the approach applied by Card et al. (2010, 2015) and Cho and Honorati (2014), we code the sign and significance for each impact estimate reported in the primary studies. This indicator of intervention success has the advantage that it is easily interpretable and neutral to the unit of the outcome variable. However, it only captures the direction and significance of an effect, unlike the standardized mean difference which preserves its magnitude (cf. Stanley and Doucouliagos 2012, p. 6). Using this approach, we construct a binary dependent variable taking the value 0 if the primary study impact estimate t-statistic is smaller than 1.96 and taking the value 1 if $t \geq 1.96$. Additionally, we extend this approach and construct an ordered categorical variable that can take three values of -1 if $t \leq -1.64$, 0 if $t \geq -1.64$ and $t \leq 1.64$, and 1 if $t \geq 1.64$. Thus, we distinguish between significant negative, insignificant, and significant positive estimates at the 10%-level because there are hardly negative estimates at the 5%-level (see Table A3 in the Appendix A).

<Table C3 about here>

We observe that mostly the sign and significance of the logged odds correspond with the model using a continuous measure of effect size reported in Table 3, column (2). However, estimated standard errors differ, as the coefficients for TOT, intensity, and mandatory are now insignificant – probably resulting from reduced variance in the dependent variable in comparison to the use of continuous effect sizes.

In column (2) we extend this approach and estimate an ordered probit model where the dependent variable consists of three ordered categories that distinguish between significant negative, insignificant and significant positive estimates at the 10%-level of financial education impact. This leads to a very similar assessment of predictor sign and magnitude as in our benchmark model in Table 3, column (2), but again slightly different estimates for the standard errors, with intensity, however, being a significant predictor in this estimation again.

GLS random effects regression. Next, we check whether controlling for unobservables affects our results. The results in column (3) show coefficients from a GLS random effects regression based on the assumptions discussed in equation 5. This estimation almost entirely matches the results of the benchmark model shown in Table 3, column (2) with the exception of an increased standard error for mandatory financial education.

Unrestricted weighted least squares. Next, we turn to an alternative unrestricted weighted least squares approach. In column (4) we weight each effect size with its inverse standard error (1/SE) and account for publication selection bias by including the standard error of each estimate as a covariate (as suggested by Stanley and Doucouliagos 2012). The results show that our results, again, largely match the results of the ordinary least squares estimations, however, the predictor for mandatory courses is now insignificant. In column (5) we redo this analysis and use the inverse variance as weights and include the variance as a covariate in the analysis to account for publication selection bias. This estimation, while qualitatively similar, shows no negative effects (due to increased estimated standard errors) for low-income clients, and mandatory courses.

Random effects meta-regression (DerSimonian and Laird 1986). Table C4 shows our preferred specification for three different estimators of random-effects meta-regression models with and without Knapp and Hartung (2003) corrected standard errors, respectively. We account for possible publication selection bias by controlling for the variance of each synthetic estimate. Using method of moments (columns 1 and 2), we find that our results are similar to our benchmark model using OLS in Table 3, column (2), with the exception of increased standard errors, especially when applying the correction suggested by Knapp and Hartung (2003), for the coefficients for low-income economies and intensity per week, which are now statistically insignificant. Turning to the alternative estimators (restricted maximum likelihood, and empirical bayes), we find that these results are again nearly identical. Overall, we conclude that the pattern in sign and magnitude (including most standard errors) of our main explanatory variables are confirmed under various random effect meta-regression models, however with a more positive assessment of the intervention impact in low and lower-middle income economies and a positive but (marginally) insignificant estimate of intensity per week.

<Table C4 about here>

(vii) Heterogeneous impacts depending on delay in measurement. In order to check for heterogeneous impacts depending on the considered time-frame, we conduct two tests. First, we model the relationship between delay in measurement and effect size on financial behavior outcomes in a non-linear fashion by creating a categorical variable that distinguishes between short term (less than one month, approx. 12% of estimates), medium term (less than one year, approx. 41% of estimates), and long-term (longer than one year, approx. 47% of estimates) effects on financial behavior. Column (1) of Table C5 shows that short term effects tend to be higher than medium- or long-term effects on financial behavior, which is in line with the present literature (cf. Fernandes et al. 2014; Lusardi et al. 2015b). Splitting the sample according to these three time-frames, we observe that most predictors are similar in sign and magnitude in all subsamples, with some differences regarding signs and significance of predictors. It seems noteworthy, and reassuring for our results, that the subsample comprising the longer-term treatment effects appears to be driving our main results. In particular, intensity appears to matter for effect sizes to be found after a long delay between treatment and measurement. This is in line with earlier observations by Fernandes et al. (2014) that intensity may interact with delay since intervention.

<Table C5 about here>

(viii) Intensity. Since the intensity of financial education supports its effectiveness, we check which aspect of intensity of education drives our results. Using only the total number of hours taught as a linear predictor of effect size (and neglecting the duration of the intervention), we find that intensity does not predict effect sizes on financial behavior (available on request). This result remains the same in several variants of variable and model specifications (e.g. including polynomial forms of intensity, interaction effects between delay and intensity, and centering) and holds when effect sizes on financial literacy are regressed on this linear predictor. Thus, the intensity relative to the duration of the intervention appears to matter most for the impact on financial behavior. This finding seems to have practical

implications, since it favors education with higher relative intensity, i.e. trainings with relatively more hours per week.

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Table C1: Financial education treatment effect on financial behavior under various models

Outcome	(1) OLS Full pooling	(2) OLS Synthetic ES	(3) RE-Metareg	(4) RE GLS	(5) WLS 1/ SE_g	(6) Robumeta
Panel A : All						
Fin. Behavior	0.086***	0.102***	0.079***	0.093***	0.026**	0.064***
	(0.012)	(0.013)	(0.009)	(0.012)	(0.011)	(0.008)
n(Studies)	90	90	90	90	90	90
n(Effect sizes)	349	90	90	349	349	349
Panel B: RCTs						
Fin. behavior	0.082***	0.102***	0.075***	0.089***	0.067***	0.078***
	(0.014)	(0.023)	(0.013)	(0.021)	(0.013)	(0.012)
n(Studies)	40	40	40	40	40	40
n(Effect sizes	227	40	40	227	227	227
Panel C: Quasi exp.						
Fin. behavior	0.093***	0.102***	0.083***	0.100***	0.015*	0.059***
	(0.022)	(0.015)	(0.012)	(0.015)	(0.008)	(0.010)
n(Studies)	50	50	50	50	50	50
n(Effect sizes)	122	50	50	122	122	122

Notes: Column (1) shows the average effect size on fin. behavior estimated via OLS with standard errors clustered by Study ID. Column (2) shows the average effect using only one synthetic (weighted average) effect size per study. Synthetic effect sizes are estimated via within-study random effects meta-regression (DerSimonian and Laird 1986). Column (3) shows the average weighted treatment effect estimated via random effects meta-regression (DerSimonian and Laird 1986) and Knapp Hartung (2003) adjusted standard errors. The Stata command is "metareg". Column (4) shows the average treatment effect of fin. edu on fin. behavior utilizing a study random-effects GLS model. Column (5) presents results using unrestricted weighted least squares where a weight of the respective inverse standard error is assigned to each observation. Column (6) presents results from robust variance meta-regression with dependent effect size estimates (Tanner-Smith and Tipton 2014). The Stata command is "robumeta". Standard errors (clustered at the study-level for Columns (1), (4), (5), and (6)) in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table C2: Missing data, subsamples and giving each study equal weight

	(1) No imputations	(2) US only	(3) Classroom only	(4) Synthetic ES OLS	(5) Equal study weights
RCT	-0.052*	-0.097**	-0.080***	-0.052	-0.042
	(0.027)	(0.042)	(0.028)	(0.033)	(0.031)
TOT	0.057	0.114***	0.065**	0.107***	0.105***
	(0.041)	(0.040)	(0.028)	(0.028)	(0.035)
Delay	-0.000*	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1/SE	0.001	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Intensity / week	0.005***	0.006*	0.004***	0.001	0.001
	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)
Duration	-0.000	-0.001	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Low income clients	-0.047**	-0.003	-0.054***	-0.043	-0.049**
	(0.020)	(0.025)	(0.017)	(0.027)	(0.022)
Years of schooling	-0.022***		-0.021***	-0.022**	-0.020**
	(0.007)		(0.006)	(0.009)	(0.009)
Low/lower-mid .econ	-0.113**		-0.108**	-0.113*	-0.108*
	(0.044)		(0.041)	(0.061)	(0.059)
Mandatory	-0.086*	-0.097***	-0.043	-0.097**	-0.095***
	(0.049)	(0.033)	(0.028)	(0.038)	(0.029)
Teachable moment	0.058	0.129***	0.075**	0.058**	0.058**
	(0.052)	(0.035)	(0.033)	(0.028)	(0.026)
Constant	0.359***	0.042	0.364***	0.364***	0.344***
	(0.097)	(0.031)	(0.095)	(0.118)	(0.119)
R^2	0.125	0.340	0.177	0.297	0.206
n (Studies)	35	55	70	90	90
n (Effect sizes)	24	135	317	90	349

Notes: Column (1) reports results for complete cases only. Columns (2) present results for the sample split of USA studies only. These splits include only variables for which differential information from at least two studies are available. Column (3) presents results using one synthetic effect size (weighted within-study average effect size across all outcomes) per study. Column (4) shows results by weighting each observation by the inverse number of observations of the study the observation is nested in. Standard errors (clustered at the study-level for all Columns but (4)) in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table C3: Alternative meta-regression models

	(1) Probit 5%	(2) Ordered probit 10%	(3) RE GLS	(4) WLS 1/SE(g) weights	(5) WLS 1/Var(g) weights
RCT	-0.794***	-0.802***	-0.087***	-0.086***	-0.044**
	(0.225)	(0.196)	(0.024)	(0.020)	(0.022)
TOT	0.052	0.002	0.049**	0.038**	0.058***
	(0.189)	(0.176)	(0.023)	(0.016)	(0.015)
Delay	-0.001**	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1/SE	0.001	-0.000			
	(0.001)	(0.001)			
SE_g			0.486***	0.611**	
			(0.173)	(0.272)	
SE_g^2					3.147**
					(1.496)
Intensity /week	0.018	0.027*	0.003**	0.003*	0.006**
	(0.014)	(0.015)	(0.002)	(0.002)	(0.003)
Duration	0.008*	-0.000	0.000	0.000	0.000
	(0.004)	(0.005)	(0.001)	(0.000)	(0.000)
Low inc. clients	-0.566***	-0.561***	-0.060***	-0.014*	-0.000
	(0.160)	(0.148)	(0.019)	(0.007)	(0.002)
Years of schooling	-0.154***	-0.136***	-0.024***	-0.022***	-0.018***
	(0.058)	(0.044)	(0.006)	(0.005)	(0.006)
Low/lower-mid. econ.	-0.872**	-0.792**	-0.105**	-0.086***	-0.076*
	(0.392)	(0.314)	(0.042)	(0.032)	(0.045)
Mandatory	0.172	0.130	-0.030	-0.026	-0.017
	(0.245)	(0.272)	(0.026)	(0.020)	(0.018)
Teach. moment	0.326	0.404**	0.063***	0.042**	0.068***
	(0.219)	(0.192)	(0.024)	(0.017)	(0.015)
Constant cut 1		-3.977***			
		(0.636)			
Constant cut 2		-1.999***			
		(0.594)			
Constant	2.009**		0.356***	0.304***	0.210***
	(0.783)		(0.079)	(0.066)	(0.079)
R^2			0.197	0.301	0.336
Pseudo R^2	0.109	0.084			
n (Studies)	90	90	90	90	90
n (Effect Sizes)	349	349	349	349	349

Notes: Dependent variable in columns (1) and (2) is a categorical indicator of sign and significance of intervention impact. Dependent variable in columns (3) and (4) is effect size (Hedges' g) on financial behavior. Column (1) reports results from probit-regression with a binary outcome indicating whether financial education had a significantly positive effect on financial behavior at the 5%-level. Column (2) provides results for ordered probit regression with a dependent categorical variable taking the value "-1" if financial education had a significantly negative impact on financial behavior, "0" if financial education had an insignificant effect on financial behavior, and "1" if financial education had a significant positive effect on financial behavior at the 10%-level. Column (3) reports results from GLS random-effects regression. Column (4) reports results of weighted least squares estimation with inverse variance weights. Standard errors clustered at the study-level in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table C4: Random effects meta-regression on synthetic effect sizes with inverse variance weights

	(1) MM	(2) MM	(3) REML	(4) REML	(5) EB	(6) EB
RCT	-0.064***	-0.064***	-0.063***	-0.063***	-0.064***	-0.064***
	(0.020)	(0.020)	(0.020)	(0.019)	(0.021)	(0.021)
TOT	0.042**	0.042**	0.041**	0.041**	0.042**	0.042**
	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)
Delay	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
SE_g^2	4.970***	4.970***	5.060***	5.060***	4.907***	4.907***
	(1.846)	(1.804)	(1.873)	(1.776)	(1.826)	(1.826)
Intensity /week	0.002	0.002	0.002	0.002	0.002	0.002
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Duration	0.001	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Low inc. clients	-0.026	-0.026*	-0.025	-0.025*	-0.026	-0.026*
	(0.016)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
Years of schooling	-0.013**	-0.013**	-0.013**	-0.013**	-0.013**	-0.013**
	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Low/lower inc. econ.	-0.034	-0.034	-0.032	-0.032	-0.036	-0.036
	(0.038)	(0.037)	(0.037)	(0.036)	(0.038)	(0.038)
Mandatory	-0.039**	-0.039**	-0.038**	-0.038**	-0.040**	-0.040**
	(0.019)	(0.019)	(0.019)	(0.018)	(0.020)	(0.020)
Teach. moment	0.049***	0.049***	0.049***	0.049***	0.049***	0.049***
	(0.017)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)
Constant	0.220***	0.220***	0.217***	0.217***	0.223***	0.223***
	(0.073)	(0.071)	(0.072)	(0.069)	(0.073)	(0.073)
I^2	79.28%	79.28%	79.28%	79.28%	79.28%	79.28%
Adj. R^2	-	-	0.532	0.532	0.565	0.565
n (Studies)	90	90	90	90	90	90
n (Effect Sizes)	90	90	90	90	90	90
Adjusted errors	yes	no	yes	no	yes	no

Notes: Results from random-effects meta-regression (DerSimonian and Larid 1986) with and without Knapp and Hartung (2003) adjusted standard errors, respectively. Dependent variable is effect size (Hedges' g) on financial behavior weighted by its inverse variance. Columns (1) and (2) show results for method of moments (MM) estimates. Columns (3) and (4) show results for restricted maximum likelihood (REML) estimates. Columns (4) and (5) show results from empirical bayes estimates. The Stata command is *metareg* (Hardbord and Higgins 2008). Standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Table C5: Effect sizes on financial behavior and heterogeneity of treatment effects by delay in measurement of treatment effects

	(1) Financial behavior	(2) Short term subsample	(3) Medium term subsample	(4) Long term subsample
RCT	-0.061**	0.148	-0.085***	-0.073*
	(0.026)	(0.102)	(0.027)	(0.038)
TOT	0.043*	-0.221**	0.043	0.062
	(0.025)	(0.078)	(0.032)	(0.049)
Short term	0.089**			
	(0.039)			
Medium term	-0.006			
	(0.018)			
1/SE	-0.000	-0.005**	-0.000	0.000
	(0.000)	(0.002)	(0.000)	(0.000)
Intensity /week	0.004***	0.006	0.002	0.004***
	(0.001)	(0.007)	(0.002)	(0.001)
Duration	-0.000	0.010**	0.000	0.000
	(0.001)	(0.005)	(0.001)	(0.001)
Low inc. clients	-0.044***	-0.046	-0.041**	-0.045**
	(0.014)	(0.087)	(0.020)	(0.019)
Years of schooling	-0.021***	-0.103**	-0.011	-0.021**
	(0.005)	(0.047)	(0.008)	(0.009)
Low/lower inc. econ.	-0.122***	-1.127***	0.034	-0.156***
	(0.041)	(0.318)	(0.055)	(0.058)
Mandatory	-0.041**	-0.076	0.003	-0.056***
	(0.019)	(0.097)	(0.047)	(0.021)
Teach. moment	0.090***	0.202*	0.009	0.109***
	(0.028)	(0.108)	(0.032)	(0.024)
Constant	0.332***	1.634**	0.235**	0.332***
	(0.077)	(0.624)	(0.101)	(0.119)
R^2	0.204	0.457	0.073	0.319
n (Studies)	90	18	24	53
n (Effect Sizes)	349	42	143	164

Notes: Results from OLS meta-regression with robust standard errors clustered at the study-level. Dependent variable is effect size (Hedges' g) on financial behavior. Standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level.

Appendix D: Publication bias and heterogeneity of study quality

We show examinations of conventional visual tests for publication bias in order to address the so-called file drawer problem (cf. Stanley and Doucouliagos 2012, p. 73) and examine the sample of studies for heterogeneous results depending on study quality. Note that we also use formal econometric methods, (i.e. alternative regression approaches) in Appendix C that are capable of generating unbiased estimates in the presence of publication selection.

Publication bias. We conduct visual tests for overall publication bias (funnel asymmetry), so-called funnel plots (cf. Figures D1 and D2). Precision of the estimated treatment effect should increase in larger studies. Thus, we scatter effect sizes (one synthetic effect per study) against the standard errors of the effect size estimates. Effect estimates from small studies (larger sampling errors) should scatter more widely at the bottom of the graph, with the spread decreasing as standard errors decrease. In the absence of bias, the plot resembles a symmetrical inverted funnel. Therefore, asymmetry indicates a publication bias in the sense that negative or non-results are under-represented (i.e. not published at all). Inspecting the two plots indicates that symmetry is higher for effect sizes on financial behavior than for effect sizes on literacy but both outcomes may be affected by publication biases in the sense that the overall treatment effect may suffer from a slight upward bias. This conclusion, however, requires the assumption that non-results are not published at all (i.e. the file drawer problem).

<Figure D1 and Figure D2 about here>

This assumption may be more plausible for quasi- and natural experiments than for RCTs, as results from rigorous randomized experiments are likely to be published irrespective of their results. Therefore, we perform the same visual check on the subsample of RCTs only (cf. Figures D3 and D4). Indeed, these plots are much more symmetric indicating that publication bias may only be an issue within the sample of non-randomized studies. As (i) nearly 40 percent of our sample is comprised of RCTs; (ii) we control for research design in all of our regressions; and (iii) our main results replicate within the subsample of RCTs, we conclude that publication biases are not an issue for our analysis. However, we also test the robustness of our results using weighted least squares and controlling for the standard error (or the squared

standard error, i.e. the variance of the estimate), which is advocated as a robust method in the presence of publication selection (cf. Stanley and Doucouliagos 2012).

<Figure D3 and Figure D4 about here>

Publication status and quality. Another concern in any meta-analysis is the issue of biases arising from the aggregation of results from studies with different publication status and quality. On the one hand, researchers fear that the tendency of the scientific community to favor statistically significant positive results over insignificant non-results may lead to biased estimates favoring the rejection of the null hypothesis of a zero-effect of financial education on relevant outcomes. The standard solution in the meta-analysis literature is to include as many unpublished studies (grey literature) as possible to address this potential source of bias *a priori*.

On the other hand, economists fear that by aggregating studies of different publication status and quality, the results suffer due to the lack of empirical rigor in grey-literature primary studies. To shed light on this issue in the financial education literature, we compare average effect sizes of financial education interventions by different types of publication status and indicators of quality. Table D1 compares average effect sizes on financial literacy and behavior by publication status in an academic journal. Interestingly, a bias affects only the effect size estimates on financial literacy, as they appear to be more than twice as high in published than in unpublished papers (t=3.863). Turning to effect sizes on financial behavior, however, we observe no significant difference in average effect sizes between published and unpublished studies.

<Table D1 about here>

Considering indicators of study quality, we code the article influence score (ISI web of knowledge) of the respective journal (and year) for every publication and assign a value of 0 for studies available as working papers. Comparing influential (article influence score >1) with less influential (≤1) publications, we find that the quality bias for financial literacy is now insignificant (t=0.328): Moreover, influential journals tend to publish studies with 0.04 standard deviation units smaller effect sizes on behavior (t=-2.189) than non-influential journals. Thus, more rigorous

work reports a slightly smaller average treatment effect than presumably less rigorous work.

Next, we code the number of citations for each publication as reported in Google Scholar (as of October 31, 2016). The mean number of citations per article is 53.91and we split the sample in studies cited above and below this threshold value. Again, we find no significant differences between highly cited studies and others: If anything, highly cited studies tend to report smaller average effect sizes on financial behavior than studies with few citations. Overall, we see that quality bias appears to be not an issue that alters the conclusions in this literature concerned with effects on financial behavior.

Table D1: Effect sizes by publication status and indicators of publication quality

Outcome	Status / Quality	Studies	Obs.	ES (g)	SE_g	p-value	Diff. (t-value)
Fin. Literacy	Published	36	106	0.343	0.066	0.000	0.179***
	Unpublished	31	85	0.164	0.039	0.000	(3.863)
Fin. Behavior	Published	50	142	0.087	0.019	0.000	0.004
	Unpublished	40	200	0.083	0.016	0.000	(0.211)
Fin. Literacy	High influence	11	36	0.247	0.028	0.000	0.020
	Low influence	56	155	0.267	0.043	0.000	(0.328)
Fin. Behavior	High influence	27	90	0.053	0.020	0.013	-0.043**
	Low influence	63	252	0.096	0.015	0.000	(-2.189)
Fin. Literacy	Highly cited	10	17	0.249	0.068	0.005	-0.016
	Few citations	57	174	0.265	0.045	0.000	(-0.195)
Fin. Behavior	Highly cited	37	73	0.070	0.024	0.006	-0.018
	Few citations	53	269	0.089	0.014	0.000	(-0.879)

Notes: ES(g) and SE_g are results from an unweighted OLS regression with standard errors clustered by study ID. Samples are split by an indicator of publication in an academic journal (published / unpublished), an indicator of high and low influence (article influence score >1), and an indicator of highly cited articles (Google scholar citations > mean(citations)).

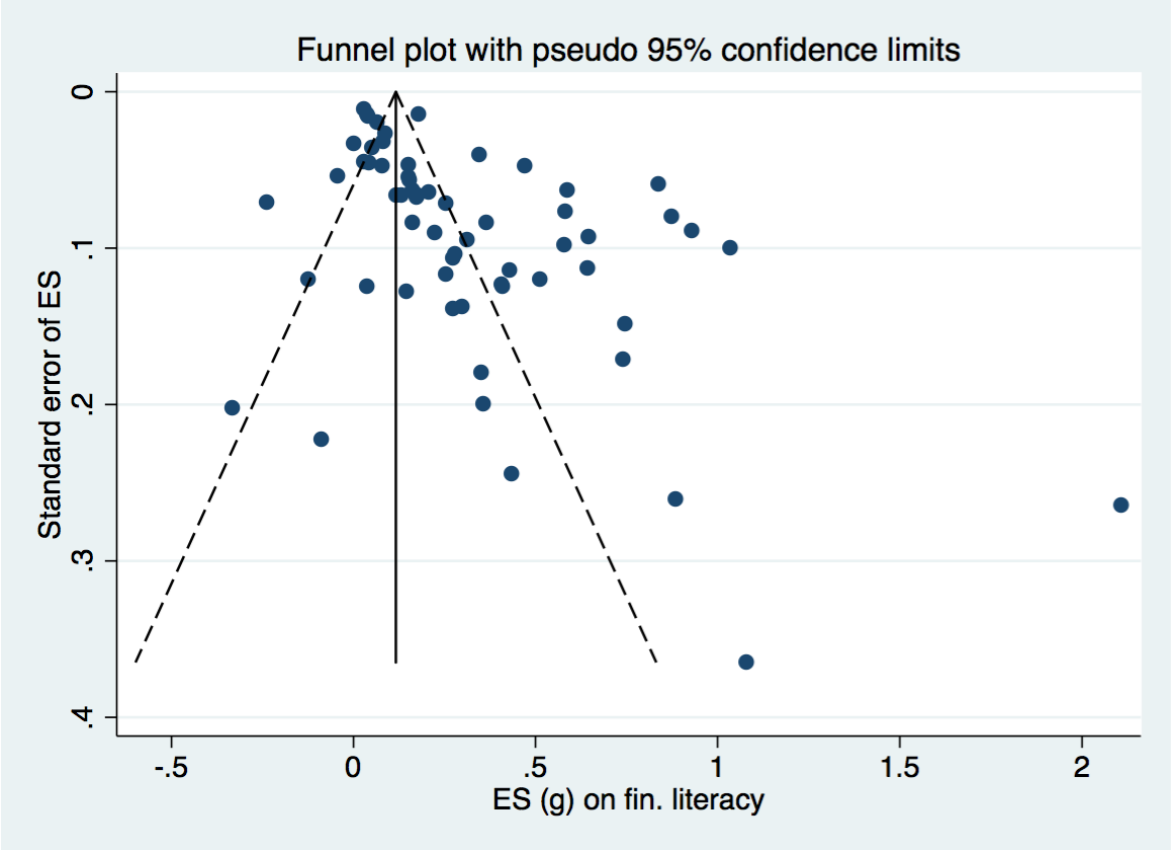

Figure D1: Funnel plot of treatment effects on financial literacy

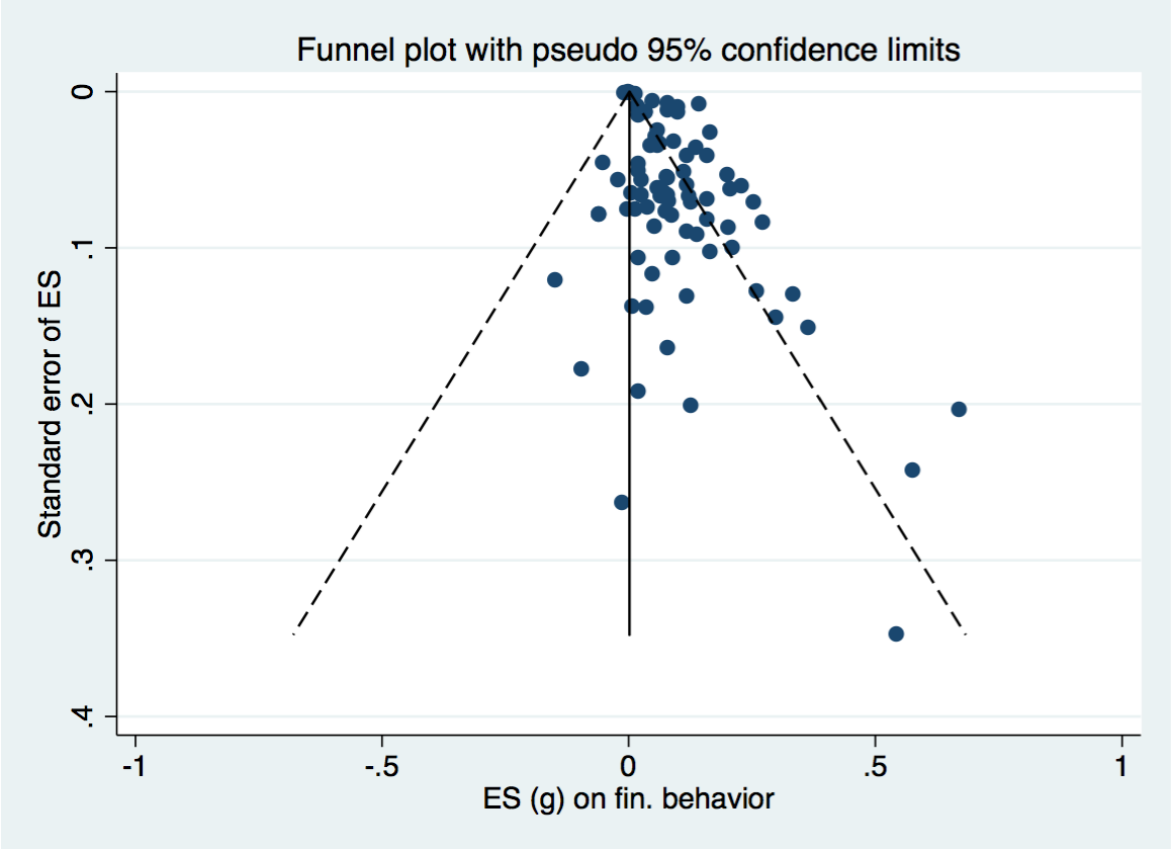

Figure D2: Funnel plot of treatment effects on financial behavior

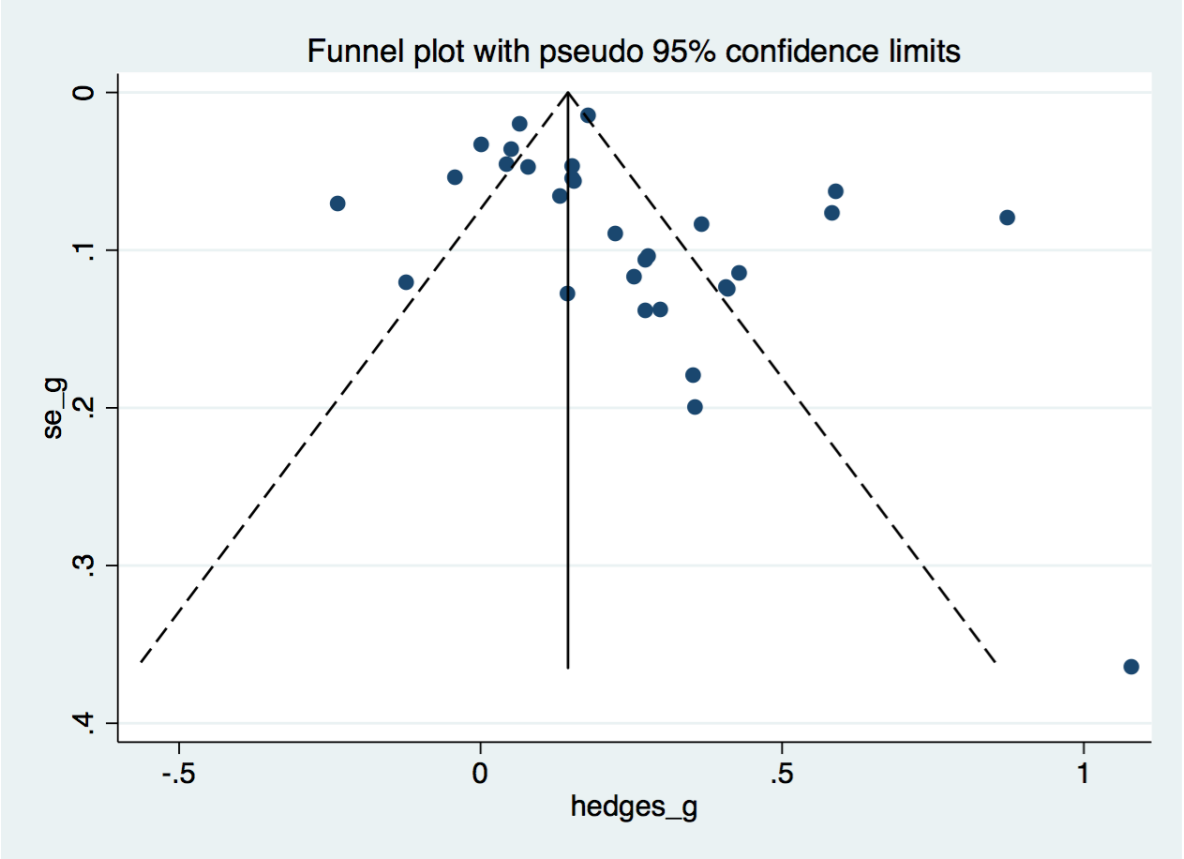

Figure D3: Funnel plot of treatment effects on financial literacy within the subsample of RCTs only

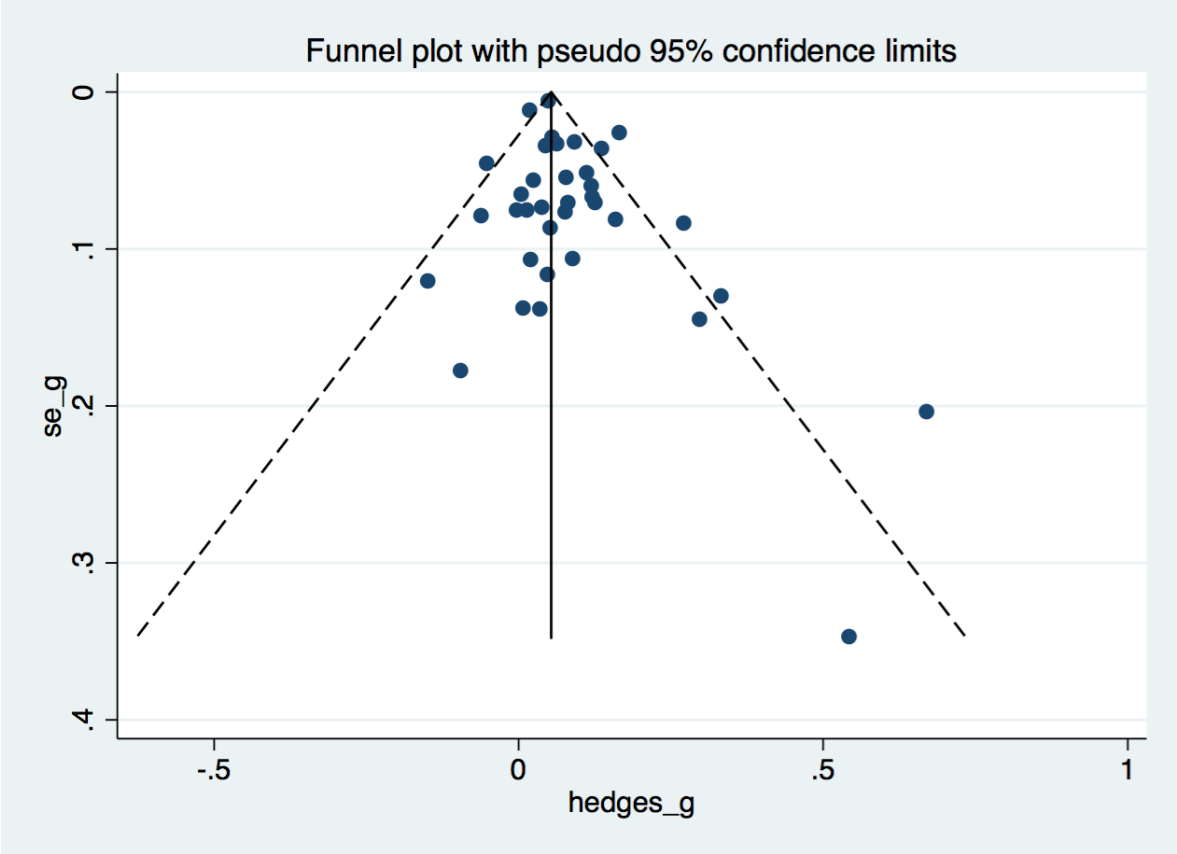

Figure D4: Funnel plot of treatment effects on financial behavior within the subsample of RCTs only

Appendix E: Overview of studies included in the statistical meta-analysis

Table E1: Overview of financial education studies included in our analysis

Study	Country	Research design	Target group		Intervention	
			Mean age	*Low-income*	*Channel*	*Teach. moment*
Agarwal et al. 2009	USA	Natural exp.	-	Yes	Counseling	Yes
Agarwal et al. 2010	USA	Natural exp.	-	Yes	Counseling	Yes
Ambuhel et al. 2014	USA	RCT	29	Yes	Online	No
Asarta et al. 2014	USA	Quasi exp.	15	-	Classroom	No
Barcellos et al. 2012	USA	RCT	52	No	Online	No
Baron-Donovan et al. 2005	USA	Quasi exp.	44	No	Classroom	No
Barua et al. 2012	Singapore	RCT	37	Yes	Classroom	Yes
Batty et al. 2015	USA	RCT	9	Yes	Classroom	No
Bauer et al. 2011	USA	Quasi exp.	-	Yes	Classroom	No
Bayer et al. 2009	USA	Natural exp.	-	No	Classroom	Yes
Becchetti et al. 2013	Italy	RCT	18	-	Classroom	No
Berg and Zia 2013	South Africa	RCT	20	Yes	Mass Media	No
Bell et al. 2009	USA	Quasi exp.	22	No	Classroom	No
Bernheim and Garrett 2003	USA	Natural exp.	39	No	Classroom	Yes
Bernheim et al. 2001	USA	Natural exp.	40	No	Classroom	No
Berry et al. 2015	Ghana	RCT	11	-	Classroom	Yes
Bjorvatn and Tungodden 2010	Tanzania	RCT	39	-	Classroom	Yes
Brown et al. 2016	USA	Natural exp.	28	-	Classroom	No
Brugiavini et al. 2015	Italy	RCT	23	No	Classroom	No
Bruhn and Zia 2013	Bosnia and Herzegovina	RCT	28	Yes	Classroom	Yes
Bruhn et al. 2014	Mexico	RCT	33	-	Classroom	No
Bruhn et al. 2013	Brazil	RCT	16	Yes	Classroom	No
Butt et al. 2008	USA	Quasi exp.	12	No	Classroom	No
Calderone et al. 2013	India	RCT	45	Yes	Classroom (video)	Yes

Carlin and Robinson 2012	USA	Quasi exp.	16	No	Classroom	No
Carpena et al. 2011	India	RCT	39	Yes	Classroom	Yes
Carpena et al. 2015	India	RCT	39	Yes	Classroom + Counseling	Yes
Chen and Heath 2012	USA	Quasi exp.	9	-	Classroom	No
Choi et al. 2005	USA	Natural exp.	40	No	Classroom	No
Choi et al. 2010	USA	RCT	31	No	Info. nudge	No
Choi et al. 2011	USA	Natural exp.	64	No	Info. nudge	No
Clancy et al. 2001	USA	Natural exp.	36	Yes	Classroom	Yes
Clark et al. 2006	USA	Quasi exp.	54	No	Classroom	Yes
Clark et al. 2015	USA	Quasi exp.	44	No	Online	No
Clark et al. 2014	USA	RCT	35	No	Info. nudge	Yes
Clark et al. 2010	USA	Quasi exp-	57	No	Classroom	Yes
Cole and Shastry 2010	USA	Natural exp.	-	No	Classroom	No
Cole et al. 2013	India	RCT	48	Yes	Counseling	Yes
Cole et al. 2014	USA	Natural exp.	17	Yes	Classroom	No
Cole et al. 2011	Indonesia	RCT	41	Yes	Classroom	Yes
Collins 2013	USA	RCT	39	Yes	Classroom	No
Custers 2011	India	RCT	34	Yes	Classroom	Yes
Danes and Haberman 2004	USA	Quasi exp.	15	No	Classroom	No
Danes et al. 1999	USA	USA	15	No	Classroom	No
De Mel et al. 2011	Sri Lanka	Quasi exp.	41	-	Classroom	Yes
DeLaune et al. 2010	USA	Quasi exp.	18	No	Classroom	No
Ding et al. 2008	USA	Natural exp.	-	Yes	Counseling	Yes
Doi et al. 2014	Indonesia	RCT	44	Yes	Classroom	Yes
Dolvin and Templeton 2006	USA	Quasi exp.	46	No	Classroom	Yes
Drexler et al. 2014	Dominican Republic	RCT	41	Yes	Classroom	Yes
Duflo and Saez 2003	USA	RCT	38	No	Info. nudge	Yes

Elliehausen et al. 2007	USA	Natural exp.	41	No	Counseling	No
ETI 2008	USA	Quasi exp.	14	-	Classroom	No
Field et al. 2010	India	RCT	32	Yes	Classroom	Yes
Fort et al. 2016	Italy	Natural exp.	-	-	Info. Nudge	No
Garman et al. 1999	USA	Quasi exp.	43	No	Classroom	Yes
Gaurav et al. 2011	India	RCT	50	Yes	Classroom	Yes
Gibson et al. 2014	New Zealand / Australia	RCT	-	Yes	Classroom	Yes
Gill and Bhattacharya 2015	USA	Quasi exp.	17	Yes	Classroom	No
Gine and Mansuri 2014	Pakistan	RCT	38	Yes	Classroom	Yes
Gine et al. 2013	Kenya	RCT	49	Yes	Edu. materials	Yes
Go et al. 2012	USA	Quasi exp.	9	Yes	Classroom	No
Goda et al. 2014	USA	Quasi exp.	45	No	Info. nudge	No
Goldsmith and Goldsmith 2006	USA	Quasi exp.	19	No	Classroom	No
Grimes et al. 2010	USA	Natural exp.	51	No	Classroom	No
Grinstein-Weiss et al. 2015	USA	Natural exp.	36	Yes	Classroom	Yes
Han et al. 2009	USA	RCT	41	Yes	Classroom	Yes
Hartaska and Gonzalez-Vega 2005	USA	Natural exp.	-	Yes	Counseling	Yes
Hartaska and Gonzalez-Vega 2006	USA	Natural exp.	35	No	Counseling	Yes
Harter and Harter 2009	USA	Quasi exp.	-	Yes	Classroom	No
Harter and Harter 2010	USA	Quasi exp.	17	No	Classroom	No
Haynes et al- 2011	USA	RCT	55	Yes	Online	No
Haynes-Bordas et al. 2008	USA	Quasi exp.	38	Yes	Classroom	Yes
Heinberg et al. 2014	USA	RCT	35	No	Online	No
Hershey et al. 2003	USA	RCT	34	Yes	Classroom	No
Hirad and Zorn 2001	USA	Natural exp.	-	Yes	Mixed	Yes

Hospido et al. 2015	Spain	Quasi exp.	15	-	Classroom	No
Jamison et al. 2014	Uganda	RCT	24	No	Classroom	Yes
Kimball and Shumway 2010	USA	Natural exp.	50	No	Mixed	Yes
Krause et al. 2016	Tanzania	Quasi exp.	-	-	Classroom	Yes
Loke et al. 2015	USA	Quasi exp.	15	Yes	Classroom	Yes
Lusardi 2002	USA	Natural exp.	-	-	Classroom	Yes
Lusardi 2005	USA	Natural exp.	55	No	Classroom	No
Lusardi and Mitchell 2007	USA	Natural exp.	53	No	Classroom	No
Lusardi et al. 2014	USA	RCT	50	No	Online	No
Lührmann et al. 2015	Germany	Quasi exp.	14	Yes	Classroom	No
Maki 2004	USA	Natural exp.	40	No	Classroom	No
Mandell 2006	USA	Quasi exp.	12	-	Classroom	No
Mandell 2009a	USA	Quasi exp.	-	-	Classroom	No
Mandell 2009b	USA	Quasi exp.	13	-	Classroom	No
Mandell and Schmid-Klein 2009	USA	Quasi exp.	16	-	Classroom	No
Mills et al. 2004	USA	RCT	36	Yes	Classroom	No
Muller 2003	USA	Natural exp.	-	No	Classroom	No
Pang 2010	Hong Kong, China	Quasi exp.	19	-	Classroom	No
Peng et al. 2010	USA	Natural exp.	35	No	Classroom	Yes
Quercia and Spader 2008	USA	Natural exp.	30	Yes	Classroom	No
Reich and Berman 2015	USA	RCT	30	Yes	Classroom	Yes
Romagnoli and Trifildis 2013	Italy	Quasi exp.	14	No	Classroom	No
Sanders et al. 2007	USA	Quasi exp.	35	Yes	Classroom	Yes
Sarr et al. 2012	India	RCT	38	Yes	Classroom	Yes
Sayinzoga et al. 2016	Rwanda	RCT	40	Yes	Classroom	Yes
Schreiner et al. 2001	USA	Natural exp.	-	Yes	Classroom	Yes

Seshan and Yang 2014	Qatar	RCT	40	Yes	Classroom	Yes
Skimmyhorn 2016	USA	Natural exp.	21	Yes	Classroom	No
Skimmyhorn et al. 2016	USA	RCT	-	-	Classroom	No
Supanantaroek et al. 2016	Uganda	RCT	-	-	Classroom	Yes
Song 2012	China	RCT	45	No	Info. nudge	No
Tennyson and Nguyen 2001	USA	Natural exp.	17	Yes	Classroom	No
Vacroe et al. 2005	USA	Quasi exp.	17	-	Classroom	No
Walstad et al. 2010	USA	Quasi exp.	18	No	Classroom	No
Wiener et al. 2005	USA	Quasi exp.	39	No	Classroom	Yes
Xiao et al. 2012	USA	Natural exp.	18	No	Classroom	No
Yetter and Suiter 2015	USA	RCT	24	Yes	Classroom	No

Appendix F: References for studies included in the statistical meta-analysis

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(ii) Experimental evidence on the causal effects of financial education among small-scale retailers in rural Uganda

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Abstract

Financial education interventions have become widespread in recent years and seek to foster the financial literacy and behavior of consumers and small-scale entrepreneurs. In seeking to address the heterogeneity in findings of the literature, the paper examines the role of differential approaches to financial education: I organize a randomized field experiment to study the effectiveness of two financial education interventions offered to small-scale retailers in Western Uganda. The treatments contrast learner-centered and teacher-centered approaches within standardized lesson-plans. Results show that financial education has modest impacts on the financial knowledge. Behavioral impacts also appear muted in most domains, however, differential treatment effects occur within the domain of saving, since the effects of learner-centered financial education on savings are relatively large.

JEL-Classification: D 14 (personal finance), I 21 (analysis of education)

Keywords: RCT, financial literacy, financial behavior, impact evaluation

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Experimental evidence on the causal effects of financial education among small-scale retailers in rural Uganda

1 Introduction

Access to financial services is expanding rapidly in developing economies. Development policies designed to advance financial inclusion in poor countries as well as technological innovations, such as mobile money (Suri and Jack 2016), have expanded the supply of formal financial products even to remote areas. While there has been substantial academic debate about the causal impact of supply-side interventions, such as micro credit and its potential role for poverty alleviation (e.g. Banerjee et al. 2015), attention recently has extended to the demand-side constraints in the financial inclusion debate: Policymakers seek to address the observation that many, especially low-income and less educated individuals, lack the knowledge and skills to use financial services to their own well-being (cf. Bruhn et al. 2016). Thus, financial education programs are expected to foster the financial knowledge and behavior of individuals to achieve financial inclusion and to promote financial stability.

While an association between financial literacy and better financial behaviors has been documented in many correlational studies (see Hastings et al. 2013 and Lusardi and Mitchell 2014 for narrative literature-reviews), the evidence on the causal impact of financial education programs on financial literacy and behavior appears much less evident. The causal effects of these interventions appear to be economically small on average (cf. Fernandes et al. 2014) and highly heterogeneous across multiple dimensions (cf. Miller et al. 2015, Kaiser and Menkhoff 2016). As even large-scale financial education programs appear to have only modest effects (cf. Bruhn et al. 2014, 2016), policymakers and researchers have moved towards evaluating alternatives to these traditional classroom-based programs (cf. Wagh 2017). While principal alternatives to an increase in financial education efforts may include a mix of policies that target individual financial behavior through behavioral nudges or an altering of the choice-architecture, interest in improving financial education programs remains high. Thus, there has recently been an increase in rigorous randomized evaluations that look at differential impacts of financial education programs with varying design-

elements. For example, Drexler et al. (2014) and Skimmyhorn et al. (2016) show that heuristics-based approaches to financial education may be more effective in impacting financial behavior than traditional curricula. Carpena et al. (2016) show in a multiple-treatment experiment that formats which combine traditional instruction with personalized elements, such as counselling and goal-setting, yield higher treatment effects on financial behaviors than instruction-only variants of the program.

Thus, I organize a cluster-randomized field experiment to contribute to this recent strand of the literature by examining the differential treatment effects of alternative approaches to financial education within classroom-based settings. Instead of varying the content through the curriculum, or exploring alternatives to the traditional classroom, the financial education treatments contrast a *teacher-centered* approach with a *learner-centered* approach within standardized lesson-plans in a classroom-setting. Two results emerge:

First, both financial education programs yield relatively modest results, overall. However, the group allocated to the learner-centered financial education program experiences a modest increase in financial literacy, self-control, and ultimately a relatively strong 33% increase in total savings relative to the control group. Thus, the learner-centered approach appears to generate larger treatment-effects than what is predicted by the current literature. However, due to relatively low power, differential treatment effects can only be confirmed for outcomes within the savings domain.

Second, I find some evidence for heterogeneous treatment effects, with the positive treatment effects on financial literacy mainly being driven by male respondents and those who are relatively educated (both have higher financial literacy at baseline). Qualitatively, this seems to indicate that the financial education programs studied appear to have positive impacts only for those with relatively high levels of financial literacy at baseline, and thus, may be ineffective for relatively less educated individuals.

This paper is structured into seven further sections: Section 2 reviews the literature on the causal effects of financial education interventions in developing economies. Section 3 describes the financial education programs studied, the experimental design, expected effect sizes and power calculations. Section 4 introduces the empirical strategy, and section 5 provides descriptive statistics, an overview over response rates, and a discussion of randomization-balance. Section 6 presents main results, an investigation of heterogeneous treatment effects and

robustness exercises. Section 7 provides a discussion of these results with respect to the previous literature. Section 8 concludes.

2 State of the literature

There is a large and constantly growing literature on financial literacy and household finance. While a robust correlation between observed variation in financial literacy among individuals and responsible financial behaviors and better outcomes has been documented in many empirical studies in both developed and developing economies (Lusardi and Mitchell 2014), the causal impact of financial education programs on financial literacy and behavior appears less clear and is subject of academic debate. Meta-analyses of the literature draw different conclusions about the treatment effect of financial education: Fernandes et al. (2014) fundamentally question the treatment effect of financial education on knowledge and behavioral outcomes and suggest that the correlation observed in non-experimental research may be entirely attributed to misspecification and omitted variables bias. Miller et al. (2015), however, perform several meta-analyses on more homogeneous sub-samples of the literature, including many interventions in developing economies, and document that financial education can be effective at targeting selected financial behaviors, especially record keeping and savings-decisions. Finally, our own and most recent meta-analysis of the most comprehensive set of experimental studies suggests that financial education has a statistically significant and positive impact on knowledge and behavior (cf. Kaiser and Menkhoff 2016). However, effect sizes are economically small (less than 0.1 standard deviation units), especially regarding debt-related outcomes, for low- and lower-middle income economies and low-income individuals (cf. Kaiser and Menkhoff 2016). Additionally, heterogeneity in effect sizes can be explained by differences in training intensity, timing, and participation conditions, with mandatory formats being associated with smaller treatment effects, and larger effects being associated with offering financial education at a "teachable moment" (cf. Doi et al. 2014, p.39). These systematic accounts of the literature suggest that treatment effects from these kinds of interventions are small, on average. This is especially evident in the context of developing economies where several rigorous experimental impact evaluations exist:

Cole et al. (2011) report that, while financial literacy predicts the use of formal financial products in a representative sample of Indonesian households, an

intervention designed to strengthen financial knowledge does not have a positive effect on opening a bank account. In a second study, Cole et al. (2013) provide evidence that a short financial education module in India has no effect on the decision to purchase formal index-based rainfall insurance. Gibson et al. (2012) document, that while a financial education curriculum target at migrants is effective in increasing the financial knowledge of participants, it did not lead to behavioral changes regarding remittances practices. However, Doi et al. (2014) show that effects of such types of programs targeting transnational households can be larger when providing financial education to both the migrant workers and the families at home.

Even large-scale interventions appear to have relatively modest impacts. Bruhn et al. (2014) report that a free financial literacy course offered to individuals in Mexico City is met with very little interest and appears to have only "minimal impact on marginal participants" (Bruhn et al. 2014, p.184). Similarly, a large scale financial education high-school program in Brazil covering almost 900 schools and over 25,000 students appears to yield mixed results. While all students show improved financial knowledge test scores (average effect of about +0.2 standard deviation units), the impact on financial behavior is mixed at best: There seems to be a small positive treatment effect on self-reported measures of savings (average effect sizes are below 0.1 standard deviation units), but the program may have led to adverse effects in the domain of borrowing and the handling of debt, since students in the treatment group reported higher usage of expensive consumer credit and even an increase in repayment-delinquencies (cf. Bruhn et al. 2016, p. 287).

Recently, the literature has shifted attention away from studying the average effect of single programs, (i) to investigating the causal chain expected to lead to behavioral change, (ii) to testing the differential impacts of multiple treatment experiments, and, (iii) to a more thorough examination of heterogeneous treatment effects. Sayinzoga et al. (2016) document the effects of a financial education program delivered to smallholder farmers in Rwanda. The training generated relatively large impacts on financial behavior (an about 0.3 standard deviation units increase in savings) and financial literacy (about 0.4 standard deviations units increase in test scores). Additionally, they investigate the causal mechanism in a two-stage regression framework and find that increases in financial literacy may be one of the channels through which financial behavior is affected (cf. Sayinzoga et al. 2016, p. 1586). This result also appears to be in line with a recent study of bank financial education policies

in Italy (cf. Fort et al. 2016) and evidence from our own meta-analysis, where effect sizes on financial knowledge and impacts on financial behavior appear to be correlated within studies (cf. Kaiser and Menkhoff 2016).

However, there also exists evidence that financial education may primarily impact behavior through alternative non-cognitive channels: Carpena et al. (2011) suggest that financial education may lead to behavioral changes through increased awareness of financial products and changes in attitudes towards financial decisions rather than increased knowledge of financial concepts. Also, Berg and Zia (2013) show that financial education messages on debt management, delivered through a popular television soap opera in South Africa, lead to an increased use of formal financial products. On the one hand, this result suggests that non-cognitive channels (in this case emotional connections) may also be important in linking financial education to behavior-change. On the other hand, those invited to watch the financial education content also showed an increase in cognitive measures of financial knowledge, leaving room for interpretation about the dominant mechanism.

While the issue of the exact mechanisms underlying the causal effects of these interventions still provides plenty of open questions, the literature has substantially advanced the understanding of the differential impact of different types of financial education formats. Heinberg et al. (2014) and Lusardi et al. (2014) present evidence from online-experiments conducted in the U.S. that interactive tools, narratives and financial education videos may be more effective than written informational content in affecting financial knowledge and self-efficacy. Drexler et al. (2014) study the differential impacts of two different financial education curricula in the Dominican Republic. They provide evidence that a heuristics-based approach, relying on simplification of complex financial concepts ("rule-of-thumb-training"), does generate larger behavioral impacts than the teaching of traditional curricula ("full technical training"), especially for low-skilled individuals and individuals with low baseline financial literacy and motivation. Skimmyhorn et al. (2016) replicate this type of experiment, comparing a "rule-of-thumb" curriculum to a "principles-based" curriculum at a military undergraduate liberal arts institution in the U.S., and, while both curricula have a positive effect on financial knowledge (the study lacks an empirical investigation of effects on behavior), they do not find evidence to support differential effects regarding the two different types of curricula, albeit in a relatively highly educated sample of respondents.

Berry et al. (2015) evaluate the impact of two different school programs, both including a voluntary after school savings-club. Both programs lead to an increase in savings. Similarly, Jamison et al. (2014) study the effects of financial education and savings-clubs in Uganda and conclude that the experimental group that received financial education and accounts have higher savings compared to the account-only group at the endline-survey. Taken together, these studies suggest that one important aspect of financial education may be the ability of respondents to immediately apply the acquired knowledge.

Finally, Carpena et al. (2016) show in a multiple-treatment experiment that formats combining traditional instruction with personalized elements, like counselling and goal-setting, yield higher treatment effects on financial behaviors, such as keeping track of household expenses through a written budget, starting informal savings, and avoiding borrowing to cope with unexpected shocks (cf. Carpena et al. 2016, p.5). They conclude that "traditional financial education programs, especially those implemented in a group setting with a one-size-fits-all approach, may be inadequate in equipping individuals with the appropriate tools to bridge the gap between financial knowledge and financial behavior" (Carpena et al. 2016, p.6).

Much of this evidence suggests that formats relying on direct instruction appear to have small effects. Thus, alternative approaches to lecture-based formats appear to be especially important when targeting low-skilled and low-income individuals. In seeking to contribute to this recent strand of the literature, we collaborate with the *German Development Cooperation (GiZ)* to organize a cluster-randomized field experiment in Uganda to study the differential impact of two alternative classroom financial education formats. Specifically, we are able to exploit the fact that *GiZ* offers two financial education formats which are standardized to cover the exact same curriculum, both having an intensity of around 2 hours. The only difference between the two classroom trainings is that one variant relies on a *teacher-centered* approach (i.e. direct instruction in a community lecture) whereas the other variant *is learner-centered.* Latter in the sense that it seeks to create individualized learning opportunities through stations where participants solve interactive tasks and are encouraged to discuss the content with their peers. I hypothesize that, although the learner-centered approach is situated in a classroom setting relying on learning facilitators, this type of classroom based training may yield larger impacts on knowledge and behavior than the teacher-centered approach.

The next section briefly describes the main features of the financial education programs and introduces the particularities of the experimental impact evaluation design.

3 The financial education interventions and experimental design

3.1 Financial education interventions

As one step towards promoting financial inclusion, the central bank of Uganda (*BoU*) has established a national strategy for financial literacy in Uganda.[1] This strategy seeks to foster the personal financial knowledge and behavior of different target groups, including school students, young adults in urban settings, and adults in rural areas. *BoU* has partnered with the *GiZ* to design effective financial education interventions. While the specific curricula and dissemination formats vary depending on the target group, they all target financial knowledge and behavior within the sub-domains of (i) budgeting and personal financial management, (ii) credit, (iii) savings, (iv) investing, and (v) payments and financial service providers.

Focusing on one of the financial education strategies' major objectives of improving the rural outreach, *GiZ* has developed educational formats for the target population of the rural self-employed. These programs teach how to create a written household budget, encourage household savings, explain the costs and benefits of credit, explain the trade-off between risk and return regarding productive investments, highlight the benefits of diversification among sources of income and investments, and inform about the benefits of using financial services provided by regulated financial institutions. Thus, these trainings promote the use of formal financial services, without generally discouraging semi-formal types of financial products (e.g. saving in village savings and loans associations (VSLAs) or rotary savings-clubs). The trainings do, however, caution against the use of expensive credit provided by informal money-lenders, and the take up of costly loans to finance consumption expenditures in general.

Currently, *GiZ* offers two different educational formats for the same target group of rural self-employed, creating the opportunity to study the differential impact of alternative delivery approaches to financial education. While these two formats are identical with regards to the content areas covered (they both address the domains (i)

[1] See: https://www.bou.or.ug/opencms/bou/bou-downloads/Financial_Inclusion/Strategy-for-Financial-Literacy-in-Uganda_August-2013.pdf; last checked on March 5th, 2017.

to (v)), they differ in their concrete delivery method. Variant A of the financial education training is designed to be *learner-centered*: Its main feature are five distinct stations, designed to provide interactive learning opportunities and encourage discussion among the participants. Variant B is organized as a community lecture, i.e. a *teacher-centered* approach, relying mostly on direct instruction through lecturing with some room for the participants to ask questions. Both variants of the financial education trainings are standardized to cover the same content and last about two hours. For the purpose of evaluating the impact of the financial education programs, we are able to employ the exact same trainers to deliver both financial education variants A and B to the target groups. Thus, the different variants are not confounded with certain characteristics of the trainers.

3.2 Experimental design

We organize a cluster-randomized experiment to study the differential impact of the financial education interventions on financial literacy and behavior. The main outcomes of interest are changes in *financial literacy* (see Appendix A for the development of a psychometrically sound measure of financial literacy) and changes in *financial behavior* within the sub-domains of budgeting, usage and handling of credit, saving and investing, as well as the use of formal payment- and other financial services. Randomization is done at the cluster-level, and 83 rural marketplaces in seven districts of the Rwenzori Region in Western Uganda form the sample of clusters considered in this study[2]. To the best of my knowledge, the dataset covers all relevant permanent and regular marketplaces in the region. Because prior information about the marketplaces (such as number of vendors and primary goods traded) is limited at the time of randomization (prior to the baseline survey), we perform an unconditional (non-stratified) randomization procedure to allocate the 83 clusters in our sample to either receive financial education treatment A (n=27) or B (n=28), or to be allocated to the control group (C) (n=28) (see Table 1 for an overview of the final sample of clusters and individuals).

< Table 1 about here>

[2] Randomization was done in Stata and is reproducible. Unfortunately, the cluster-level dataset contained one duplicate cluster (market) which was known by two different names in the local languages. Thus, randomization was erroneously done with 84 markets. This error was discovered only after randomization and initial field activities. The duplicate (which was allocated to group A) was removed ex post.

3.3 Expected effect sizes and power-calculations

As mentioned in section 2, meta-analyses of the literature indicate that treatment effects of financial education are small. This is challenging for any experimental impact evaluation, since relatively large sample sizes are needed to have adequate power to reject the null hypothesis of a zero-effect. This is especially evident in cluster-randomized experiments, since the effective sample size is not only determined by the total number of observations, but rather dependent on the intra-cluster-correlation (ICC) of the outcomes within clusters. At the extreme, outcomes are perfectly correlated within clusters ($\rho = 1$), and thus, the effective sample size reduces to the number of clusters. The other extreme scenario would be to observe no ICC for the outcomes ($\rho = 0$), hence the experiment would be powered as if randomization and treatment would have occurred at the individual level. Previous studies have found ICC for rural savings programs to vary between ($\rho = 0.01$) and ($\rho = 0.1$). As I was only able to estimate ICC for our outcomes after the baseline survey (and, thus, too late for ex-ante power calculation), I used these parameters to estimate minimum detectable effect sizes for our design at a significance level of 5% and 80% power (i.e. 20% chance of type-II error).

< Figure 1 about here>

The impact of ICC on power is quite substantial. Figure 1 shows power and minimal detectable effect sizes for three scenarios of ICC. Assuming an ICC of 0.01 with 83 clusters and an (constant) cluster size of n=16 would result in a minimum detectable effect size of about 0.15 standard deviations. This is around 50% higher than the average effect on savings in the literature and about half the size of the average treatment effect on financial literacy. Allowing ρ to increase to 0.05, increases the minimum detectable effect size to 0.20. And at $\rho = 0.1$ the experiment is powered to detect effect sizes as small as 0.24 standard deviation units.

How do these minimal detectable effect sizes compare to what can be expected from the present literature? Using the meta-regression model described in Kaiser and Menkhoff (2016), I am able to predict the expected effect sizes by plugging the features of research design, intervention details, setting, and target group into the model. The model predicts an average treatment effect of 0.15 standard deviation units on financial literacy (SE=0.08, p=0.07) and insignificant treatment effects on financial

behavior in general (+ 0.003 standard deviation units with SE=0.03 and p=0.931). Thus, in order to identify meaningful treatment effects, the trainings evaluated have to be substantially more effective than similar trainings documented in the present literature.

4 Empirical strategy

To estimate the causal effect of the financial education interventions on financial literacy and behavior, I compare the two treatment groups (A) or (B) to the control group (C) at the time of follow-up. Because selection into treatment was done through randomization and section 5.2 indicates balanced groups at baseline, the unbiased *intent-to-treat (ITT)* treatment effect (average effect of being assigned to a treatment-cluster) can be estimated within the following OLS framework:

$$y_{ic(t)} = \alpha + \beta_1 A_c^T + \beta_2 B_c^T + \varepsilon_{ict} \qquad (1)$$

Here, $y_{ic(t)}$ denotes the outcome (measure of financial literacy or behavior) for individual i in cluster c at the time of follow-up (t). A_{ic}^T and B_{ic}^T are dummy variables equal to one for respondent situated in a cluster being assigned to treatment A or B, respectively. Thus, β_1 captures the (ITT) treatment effect of financial education intervention A, and β_2 captures the (ITT) treatment effect of intervention B. ε_{ict} denotes the error-term. Standard errors are clustered at the market-level to account for the level of randomization. Because the dataset includes observations for y_{ic} both at baseline ($t-1$) and follow-up (t) one can improve the model to the following single-difference specification which controls for the value of the outcome at baseline ($y_{ic(t-1)}$):

$$y_{ic(t)} = \alpha + \beta_1 A_c^T + \beta_2 B_c^T + \delta_1 y_{ic(t-1)} + \varepsilon_{ict} \qquad (2)$$

Finally, one may improve precision by controlling for observable baseline-covariates, included in the vector X_{ij} and/or district-level fixed-effects θ_d:

$$y_{ic(t)} = \alpha + \beta_1 A_{ic}^T + \beta_2 B_{ic}^T + \delta_1 y_{ict-1} + \delta_j \sum X_{icj(t-1)} + \theta_d + \varepsilon_{ict} \qquad (3)$$

For binary outcomes, linear probability models are used (see Karlan and Valdivia (2011), Cole et al. (2013) and Drexler et al. (2014) for an identical approach). However, results are not sensitive to changing the estimations to non-linear (logit or probit) models for binary outcomes (available on request).

Finally, to avoid problems inherent to testing multiple hypotheses (type-I-error inflation), I aggregate multiple related outcomes into index-measures of outcomes families: Following Kling et al. (2007), Karlan and Valdivia (2011), and Drexler et al. (2014), I define $y^*$to be an equally weighted average z-score index of its components y_k^*. Thus, for each component of a given outcome-family, I first rescale each outcome such that positive values indicate desirable treatment effects. Next, I standardize the outcome to have a mean of zero and standard deviation of one for the control-group: $y_k^* = \frac{y_k - \mu_k}{\sigma_k}$, with μ_k denoting the mean of y_k for the control group (C) and σ_k denoting the standard deviation of y_k for the control group. The aggregate index then takes the following form: $y^* = \frac{\sum_k y_k^*}{k}$.

The tables presented in section 6.1 discuss main treatment effects based on the models (1) to (3) using these aggregate indices. Whenever noteworthy, individual components are examined. Section 6.2 discusses potential heterogeneous treatment-effects where quantile-regressions are used to estimate the average treatment effect at certain points in the distribution of the outcome. Additionally, I present evidence on heterogeneous treatment effects for selected subgroups within our sample.

5 Data

After mapping of the markets, piloting the survey tools and interventions, and randomization, we conducted a comprehensive baseline survey between November 1st and December 19th of 2015. This dataset covers all vendors invited and interested to participate in our survey (n=1,291) and includes information on 245 survey-items. The questionnaires were translated into three local languages widely spoken in the area, and enumerators were trained extensively prior to the field-activities. Vendors were mobilized to participate in our survey and the financial education sessions by the local market-chairpersons. The treatment status was unknown to the market-chairperson, so no differential selection (mobilization) should be in place due to the reliance on local market-chairpersons. However, selection biases could arise when market-chairpersons favor specific groups (such as their peers) over others in general. This would indeed impact the *external* validity of our experiment in the sense that treatment effects may

causally interact with unobserved features of our specific context and sample (cf. Muller 2015); the *internal* validity, however, is not affected when these selection biases occur in all three groups. I do, however, take this concern seriously. Future steps for an evaluation of this specific sample selection issue are discussed in section 7. After baseline survey, the treatment groups received either financial education treatment A or B on the same day (right after the interviews). The control group (C) did not receive any treatment.

5.1 Baseline descriptive statistics

Table 2 reports summary statistics for the full sample and each experimental condition at baseline. *Panel A* shows variables that measure characteristics at the household level. The average household size is 6.83 people, with an average of 2.17 adults contributing to the household's income and a mean of 4.17 children being supported. Several currency denominated outcomes had a long right tail, possibly indicating enumeration errors. Following Blattmannet al. (2015), I winsorize all currency denominated outcomes at the 99th percentile. The mean (winsorized) monthly household income is around 220,000 UGX (about 60 USD). The mean (winsorized) monthly household consumption value is about 593,000 UGX, thus two to three times higher. This difference is mainly due to activities in subsistence farming. While the survey did not explicitly include an item about food-self provisioning at baseline, nearly 83% of the sample report to be engaged in subsistence agriculture at the time of the follow-up survey. Another potential source driving the discrepancy in reported cash-income and consumption may be seasonality of income: While we asked for the total income for the last 90 days (allowing to list multiple sources), we captured total consumption per week, month, and the whole year (for different types of consumption goods), and averaged these responses into a single monthly consumption value. Thus, the two variables do not exactly refer to identical time-frames.

Panel B reports variables at the respondent-level. Our sample is predominantly comprised of females (80%) and the average age is 36.23 years. On average, participants have been vending goods on markets for 7.42 years. Only 14% report to be selling non-food items (mainly second-hand clothing). The other 86% of the sample sell either fresh agricultural products or prepared food. Over two thirds (68%) are able to read and write in any language and about 25% participated in education beyond primary school. About two thirds of the sample reports to be married. 70% of the

respondents state that they are the main contributors of income to the household, while 55% report to be the "head of the household". Only 16% report to be economically dependent on others. 22% of the sample, however, receive aid or assistance from other NGOs or government programs.

We elicit general and domain-specific risk attitudes using common non-incentivized survey items developed by Dohmen et al. (2010, 2011).[3] These survey-items ask for willingness to take risk on a 0 to 10 scale. On average, respondents are relatively risk-averse. This applies both to the general risk attitudes (mean of 3.61) and to risk attitudes regarding the financial domain (mean of 3.78)[4]. The modal and median are at 3 for both the general and domain-specific case.

Finally, our survey includes a measure of numeracy and several psychological variables which are standardized into z-scores to have a mean of zero and a standard-deviation equal to one in the pooled sample.

< Table 2 about here>

5.2 Randomization balance

Causal inference within the estimation framework introduced in section 4 rests on the random assignment of cluster to the treatment conditions which achieves balanced observed and unobservable characteristics. To probe randomization balance, columns (4) and (6) of table 2 report the difference in means between the control group and the treatment groups. These differences are estimated within a simple regression framework, where standard errors are clustered at the market-level. Despite randomization, small differences exist: In group A, a smaller share of the households seem to own the dwelling they live in. However, this difference is only marginally significant. Second, the treatment groups are estimated to be slightly younger, on average, than the control group. However, again, this difference is only significant at the 10%-level. Thus, the only difference that is estimated to be statistically significant at the 5%-level is the number of years he or she works as a market-vendor. On average, respondents in the control group have been vending for about 2 years longer than their

[3] Initially, we included incentivized "lab-in-the-field experiments" to elicit risk preferences. However, we experienced several issues with interviewer-non-compliance and fraudulent behavior. Thus, I do not include this data in the final analysis.

[4] To put this result into perspective: Menkhoff and Sakha (2017) report averages of 6.85 and 6.47 on the exact same items in a survey of over 700 households in rural north-eastern Thailand, and Dohmen et al. (2011) report the modal response to be 5 in a representative sample of the German population.

counterparts in group B. These minor imbalances are what can be expected to occur by chance. Reassuringly, joint tests of orthogonality where an indicator of being assigned to group A or B is regressed on all observable baseline covariates result in low explanatory power and p-values greater than 0.1 in both cases. Thus, orthogonality and balance seems to be met in this sample of 914 respondents (377 respondents have missing values on at least one of the covariates included in this regression).

5.3 Response rates

After baseline-survey and subsequent financial education treatments, we conducted follow-up surveys between April 6th and July 19th of 2016. The follow-up survey covers 209 items and is mostly identical to the baseline survey. After this first round of tracking efforts, we had followed-up with 1,094 vendors (i.e. the attrition rate was at 15.26%). To minimize attrition, we undertook extensive tracking efforts to follow up with another 59 respondents in October 2016 and another 9 respondents during a last tracking activity in February 2017. Thus, our final response rate is relatively high: We followed up with roughly 90% of the initial sample at the end of the follow-up wave. Unfortunately, attrition rates vary by experimental condition: While the control group (C) and treatment group (A) have attrition rates of 8.55% and 7.25%, respectively, the attrition rate in treatment group (B) is almost twice as high as in group (A) (14.25%). This indicates selective attrition which may have a biasing effect on the treatment effect estimates. Thus, the issue of selective attrition is examined in greater detail in section 6.3.

6 Results

This section reports on the main treatment effects of the two financial education interventions (6.1) as well as heterogeneous treatment effects for subsamples and at various points of the outcomes distributions.

6.1 Main treatment effects

Table 3 reports the average (intent-to-treat) treatment effects of the financial education variants (A) and (B) on financial literacy scores and five domains of financial behavior. *Panel A* shows unadjusted OLS results. *Panel B* includes the lagged dependent variable at baseline and the full set of baseline covariates described in Table 2. Both panels include district-fixed-effects to account for district-level unobservable

characteristics (cf. section 6.3 of a discussion of district-level events that suggest including these district dummies).

< Table 3 about here >

Impact on financial literacy. Column (1) shows the causal effect of financial education on financial literacy scores (cf. Appendix A on how FL is measured). Intra-cluster-correlation (ICC) for this outcome is relatively high at baseline ($\rho = 0.11$). Thus, the minimum detectable effect size (at $\alpha = 0.05$ and 80% power) is about 0.25 standard deviations. Treatment A is estimated to have increased financial literacy scores by 0.15 standard deviations, however, the standard error is relatively large and the result appears to be only marginally significant (which may be expected due to low power for this outcome). At an effect size of 0.15, our experiment has a 60% chance of type II error. The magnitude of the coefficient and its standard error are, however, exactly in line with the predictions of the meta-regression model in section 3.3. Turning to treatment B, the effect size appears to be roughly half the size and statistically insignificant. Despite the relatively large difference, an F-test testing the equality of these coefficients (or testing H_0: A – B = 0) cannot reject the equality for the estimated treatment effects. The estimates reported in *Panel B* show smaller effect sizes and insignificant treatment effects in general, while preserving their relative difference magnitude. Note that the estimations in *Panel B* work with less information due to missing observations in the vector of baseline covariates.

Impact on budgeting behavior. ICC is low for this outcome family ($\rho = 0.02$), so the experiment has power to detect effect sizes below 0.2. However, the treatments appear to not have an effect different from zero on budgeting behavior. While the result of the single difference estimation reports an effect of 0.09 standard deviation units on budgeting behavior for treatment A, this coefficient appears to be only marginally significant.

Impact on borrowing behavior. ICC for this family of outcomes is estimated to be around 0.05 at baseline. Thus, the experiment is powered to detect effect sizes around 0.2 standard deviations. However, the trainings appear to have no impact on borrowing practices and total debt. While both trainings appear to have negative signs on total debt, the coefficients on the debt index are also negative (indicating adverse effects), small and insignificant from zero. The estimates in *Panel B* show that

treatment B may have a negative effect on reporting any debt at follow-up. But with the caveat that the sample, again, is substantially reduced. In total, both treatments have no effect on borrowing-related behaviors. This, however, is in line with average effect sizes in the literature (cf. Miller et al. 2015, Kaiser and Menkhoff 2016).

Impact on saving behavior. While the treatments so far appear to have very modest effect on literacy, and no effects on budgeting practices and borrowing, columns (6) to (9) show that treatment A appears to have a relatively strong effect on saving behavior. Starting with the savings index (column 6), treatment A is estimated to have an effect size of 0.16 standard deviation units ($p < 0.05$), while treatment B is estimated to have an effect size of only 0.04. Even in *Panel B* the treatment is estimated to have an effect around 0.12 standard deviation units, however, only of marginal statistical significance. In line with the strong effect on the savings index is a modest effect on the probability to report any savings at follow-up. The main index components driving the positive treatment effect, however, are strong increases in total savings and net savings. Respondents in treatment group A report an increase of 175,566 UGX in total savings which amounts to a treatment effect of approximately 0.18 standard deviation units, or an increase in savings by 33% over the mean of the control group. Even when controlling for baseline covariates the treatment effect is estimated to be roughly 0.15 standard deviation units and to be significant at the 10%-level. In contrast, the treatment effect of treatment B is estimated to be around 0.02 standard deviation units, and, statistically insignificant from zero. Testing for differential impacts, however, again does not allow rejecting the hypothesis for equality of coefficients. The effect on net-savings appears to be even stronger. This variable captures the negative coefficient on total debt (which was insignificant in the estimation in column 5) together with the positive treatment effect on total savings. Thus, this effect appears quite strong and statistically significant at the 5% level, both in the simple OLS estimations reported in *Panel A* and in the single-difference models reported in *Panel B*. In the case of net-savings, equality of treatment effects for A and B can clearly be rejected in both models ($p=0.04$).

Impact on investing into own business. Turning to total investments into the own business, both treatments appear to yield statistically insignificant effects. The effect sizes are estimated to be around 0.17 for treatment A and 0.06 for treatment B, and thus similar to the case of savings. ICC, however, is extreme for this outcome ($\rho =$

0.15). Thus, standard errors are relatively large, and the confidence interval does include the possibility of zero effects.

Impact on the use of payment and other services. Finally, I look at treatment effects on a financial services index. The components are questions on the use of mobile money, formal insurance, and the confidence to ask financial services providers for advice. Column (11) reports on this type of outcome. Both treatments appear to have effects on the use of services, however, only treatment B appears to have a relatively strong and robust effect of about 0.1 standard deviations. Equality of treatment effects, however, cannot be rejected.

6.2 Heterogeneous treatment effects

Turning to an investigation of potential heterogeneity in treatment effects, I examine the possibility of heterogeneous treatment effects for three subgroups which generally are known to have different levels of ex-ante financial literacy (cf. Lusardi and Mitchell 2014). First, gender differences are treated as a stylized fact in the literature, with men scoring higher on financial literacy tests than women in most surveys. Second, financial literacy is correlated with general educational attainment. In our sample, nearly 32% cannot read or write in any language, thus differential impacts conditional on general literacy may occur. Third, financial literacy is reported to be especially low among the relatively young, thus I compare those in their 20s with individuals over 30 years of age.

< Table 4 about here>

The three panels of table 4 show an investigation of heterogeneous treatment effects for the subgroups discussed above. In each panel, binary indicators for each group are interacted with the treatment dummies to estimate the heterogeneous effects. The results are mixed: Starting with the impact on financial literacy scores (column 1), it can be observed that treatment effects of treatment A appear to be heterogeneous along all three dimensions. First, the positive treatment effect on financial literacy may be driven predominately by a very strong treatment effect on males' financial literacy scores (0.049 + 0.377=0.426) whereas the treatment effect for females appears to be small and insignificant (*Panel A*). Second, treatment effects appear to be relatively strong for those who are able to read and write only (-0.110 + 0.329 =0.219). In

contrast, the treatment effect for those who are illiterate is estimated to be negative and insignificant from zero. Third, treatment effects appear to be zero for those under 30 years of age, while the treatment effect of those over 30 years of age, again, is estimated to be around 0.2 standard deviation units. Thus, all three examinations hint at the same qualitative implication: Treatment effects on financial literacy may be conditional on prior education and higher than average baseline-levels of financial literacy.

Turning to outcomes with regard to behavior, however, indicates that treatment effects appear to be less heterogeneous. Apart from strong negative interaction effects of the male indicator with the treatment dummies (suggesting males have even worse outcomes on the debt index than females) none of the interactions appear to be statistically significant and meaningful. One exception may be that, again, those who are able to read and write may be driving the positive treatment effects on the "use-of-financial-services-index". This time, only for those in treatment group B.

6.3 Robustness

This section mainly deals with the issue of selective attrition. In the following, I analyze determinants of attrition and examine the implications of attrition on the estimated treatment effects by estimating bounds for the treatment effects (A) and (B) under a plausible scenario for outcomes of those who are not met at the follow-up survey.

Selective attrition. As mentioned in section 5.3, attrition varies between the treatment and control groups. It appears that there is a significant effect of being assigned to treatment B on attrition. To investigate this issue, I first look at attrition per district: Just before our follow-up survey in April 2016, the districts of Bundibugyo and Kasese experienced violent outbreaks related to disputed local elections and tribal differences. These violent episodes lead to several deaths, military involvement and, according to local media, displacement of several thousand individuals from the district of Bundibugyo. 22 out of 83 markets are located in this district (and another 12 markets in the district of Kasese). These clusters make up roughly 39% of individuals in our entire sample at baseline. Because randomization at the cluster-level was not stratified by district, the distribution of treatment groups along these clusters is heterogeneous: In the case of Bundibugyo, only 23% of clusters belong to the control group. Thus, 77% of clusters have been assigned to financial education treatment in

the district. Out of these, 52% have been assigned to treatment B which experiences the highest attrition. Thus, attrition appears to be driven by this exogenous shock.

To investigate further determinants of attrition, I estimate a linear probability model of being absent at follow-up on selected baseline covariates which have a relatively small number of missing values. In a second step, I test for differential determinants depending on the treatment group by interacting these baseline covariates with the treatment dummies.

< Table 5 about here >

Several characteristics are systematically related to dropping out from the survey. In general, being allocated to treatment B is a positive determinant of attrition, even when controlling for other baseline covariates. Column (1) of table 3 shows that, in general, females are about 5 percentage points less likely to drop out. Also, a negative coefficient on age indicates that younger respondents are more likely to be absent at follow-up. However, the marginal effect of a one year increase in age is only estimated to be a 0.3% increase in the probability to drop out. Finally, there appears to be a negative effect of the total monthly household consumption on the probability to be absent at baseline. However, this effect, while statistically significant at the 5%-level, is economically miniscule: A 100,000 UGX increase in household consumption (an almost 50% increase over the mean) would only result in a 0.4% decrease in the probability to be absent at baseline.

Turning to differential determinants for the treatment groups, it appears that none of the interaction terms between the indicator for being assigned to group A is statistically significant at the 5%-level. The interaction effects between the indicator for treatment B and baseline covariates indicate that most of the determinants of attrition appear to be driven by characteristics of this group. The linear predictors mostly lose explanatory power, and, despite effects of monthly income heading into opposite directions for the treatment groups and the control group (they are economically very small), heterogeneous effects on attrition may be present in group B. In group B, age is a strong negative predictor of being absent at follow-up, and those who are literate are also less likely to drop out. Thus, we may be missing young and less educated respondents in this group. Depending on the assumptions of the average treatment effect in this group this may lead to an upward (or downward) bias.

Note, however, that these models explain only 5 and 7% of the variance in attrition. Nevertheless, there is evidence for selective attrition. Especially, there seems to be a treatment effect of being assigned to treatment B on dropping out of the survey, even when controlling the exogenous district-level shock through district dummies and when controlling for other baseline covariates.

Bounds estimates for the financial education treatments. To address selective attrition, I estimate bounds for the worst-case scenarios for the treatment effects on the financial literacy and savings.

<Table 6 about here>

Similar to Karlan and Valdivia (2011) and Drexler et al (2014), I follow Horrowitz and Manski (2000) and Lee (2002), and impute plausible values for missing observations to estimate bounds for the treatment effect. For the lower bound, I impute the minimum value of each outcome variable in the observed distribution of y to the attriters in the treatment groups, and the maximum value of the observed control distribution to the attriters in the control group. For the upper bound, I impute the maximum value of each outcome variable in the observed treatment distributions to the attriters in the treatment groups, and the minimum value of the observed control distribution to the attriters in the control group (cf. Karlan and Valdivia 2011, p. 522). Thus, column (1) shows the lower-bound estimate for the worst-case scenario of selection bias, and column (3) shows the upper-bound estimate for the worst-case scenario. Column (2) shows the unconditional treatment effect. All coefficients are estimated within a single-difference framework and include district-fixed-effects.

Turning to the worst case lower-bound scenario for treatment effects on the savings index, column (1) of *Panel A* shows that both treatments are estimated to have negative effect, which appears to be even significant at the 5-% level in the case of treatment B. Thus, equality of treatments can be rejected by an F-test (testing $A - B = 0$). Turning to the worst case upper bound, both coefficients are estimated to be positive but only the effect of treatment A is large and statistically highly significant at the 1%-level. The coefficient B is estimated to be roughly one third of the size of treatment A (0.7 vs. 0.21 standard deviation units) and statistically insignificant. Thus, the differential treatment effect on the savings index can be confirmed regardless of

the extreme missing data scenarios. Treatment A is always estimated to have larger (or less adverse) effects on savings.

Panel B shows the lower and upper bounds for the worst-case scenario regarding the treatment effect on financial literacy. The lower bound worst case-scenario (column 1) shows large negative effect sizes, statistically significant only for treatment B. Again, treatment A is estimated to have less of an adverse effect in this scenario. The test of equality of coefficients is rejected. In the upper bound scenario both treatments are estimated to yield strong effects on financial behavior (around 0.4 standard deviation units), and the effect of treatment B is now even estimated to be stronger than the effect of treatment A. However, equality of treatment effect coefficients cannot be rejected in this scenario. Overall, these examinations show that selective attrition may have a substantial impact on the estimated treatment effects. These scenarios are extreme by design, and thus, unlikely to be at work in the sample of attriters. Instead, the results are relatively reassuring, since the main results regarding the differential impact of the two treatments on savings appear to be robust to imputing missing values according to either one of these extreme scenarios.

7 Discussion

Overall, the results indicate that both financial education programs have modest effects. But, these results have to be put in perspective to previous studies in the literature. As noted in section 3.3, the meta-regression model described in Kaiser and Menkhoff (2016) predicts an average treatment effect of 0.15 standard deviation units on financial literacy (SE=0.08, p=0.07) and insignificant treatment effects on financial behavior in general (+0.003 standard deviation units with SE=0.03 and p=0.931).

While the teacher-centered approach (B) appears to have smaller impacts on financial literacy than predicted, the learner-centered approach (A) appears to have an effect of roughly the magnitude that was predicted, albeit with a larger standard error. Turning to the behavioral impacts, the learner-centered approach appears to generate substantially larger impacts than previous studies in the literature. The effect on savings behavior is relatively strong (above 0.1 standard deviations) and estimated with relatively high precision. However, due to low power, rejecting the null-hypothesis of equality among the two financial education programs mostly fails in the estimations reported in table 3. As can be seen in column (2) of table 6, estimating single-difference models without including other baseline covariates improves power

and keeps the number of observations high and allows rejecting the equality of treatment effects for both programs for the savings index, total savings, and net savings. Therefore, this paper presents strong evidence for differential treatment effects on savings behavior, after all.

How could the learner-centered financial education program have led to these relatively large changes in savings behavior? Given that the training does not generate large increases in financial literacy, the reason for the size in behavioral impacts remains puzzling. Especially, because heterogeneous treatment effects on financial literacy are strong. Training for females, the illiterate, and the young seems to have no (or miniscule) effects on financial literacy. Regarding savings behavior, no such interaction-effects exist and treatment effects are relatively strong even in groups that do not experience an increase in financial literacy in response to the training.

Trying to understand the mechanism, I perform some exploratory analyses that are not part of the pre-analysis plan, and hence, should be viewed as a hypothesis generating activity rather than experimental results. First, learner-centered financial education appears to yield a small (marginally significant) effect on a measure of self-control (+0.14 standard deviation units, SE=0.08), whereas the teacher-centered approach has no effect (-0.06 standard deviation units, SE=0.09). Second, although both trainings appear to have no effect on income, learner-centered financial education appears to have reduced monthly consumption expenditures by about 3%. This difference is not statistically significant. Third, as indicated by the negative signs on total debt and strong positive effects on net savings, learner-centered financial education may have nudged the respondents to reduce the amount of outstanding debt and may have helped to save money on costly interest payments.

Overall, these exploratory insights appear to highlight that the effect of learner-centered financial education may be largely driven by an increase in self-control and motivation to change some household financial behaviors. Thus, this training may affect behavior primarily through a *non-cognitive* channel.

This interpretation has a caveat: If impacts are driven by changes in temporary self-control and motivation, a sustained impact may be questionable. Thus, it will be important to measure the impact after a longer post-treatment period.

Certainly, this study is not without limitations. First, due to selective attrition and unexpected events (cf. section 6.3), estimates may be biased in either direction.

However, the robustness exercises indicate that the main qualitative conclusions may still hold even under a worst-case scenario.

Second, I am currently unable to give answers to the question of external validity. While the comparison with results from meta-analysis and future integration of the results of this study into meta-analyses may contribute to understanding the heterogeneous effects of financial education programs across settings, it is unclear whether the treatment effects in the study causally interact with unobserved features of the sample and context. Regarding the possible selection effects in the sample, it is planned to utilize pre-treatment data of randomly selected vendors in treatment clusters that were not mobilized into treatment. This data can be used to investigate whether respondents mobilized by market-chairpersons are systematically different from those who did not end up in our dataset. Finally, since these individuals are untreated subjects in treated clusters, it may be worthwhile to investigate the possibility of intra-cluster-spill-overs. In addition to that, since market vendors are mobile and migrate to other market-places within our study area, the question of inter-cluster-externalities remains valid. However, previous studies that looked at possible externalities from financial education have not found strong evidence for spill-overs (cf. Drexler et al. 2014, Sayinzoga et al. 2016).

8 Conclusion

This paper has used a cluster-randomized field experiment to evaluate the differential treatment effects of two alternative approaches to financial education within classroom-based settings. The study has contrasted a *teacher-centered approach* with a *learner-centered* approach within standardized lesson-plans in a classroom-setting. Both financial education treatments yield muted results. Overall, a clear picture emerges: The two financial education programs appear to yield differential effects. The *learner-centered* approach has a positive impact on financial literacy, self-control, and savings. This result is robust even when accounting for heterogeneous treatment-effects and selective attrition. The *teacher-centered* approach, in contrast, has no effect on any of these outcomes. However, due to low power, the concrete mechanisms behind these differential impacts are not yet fully understood. More rigorous evidence from randomized controlled trials and replication in other contexts is needed to probe these mechanisms and external validity of these findings (cf. Hastings et al. 2013).

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Table 1: Sample Overview

	Wave	Control group	Treatment groups		Total
			Treatment A	Treatment B	
Clusters (marketplaces) (%)	Baseline	28 (33.73%)	27 (32.54%)	28 (33.73%)	83 (100%)
	Follow-up	28 (33.73%)	27 (32.54%)	28 (33.73%)	83 (100%)
Individuals (%)	Baseline	456 (35.32 %)	414 (32.07%)	421 (32.61%)	1,291 (100%)
	Follow-up	417 (35.89%)	384 (33.05%)	361 (31.06%)	1,162 (100%)
	Attrition (individuals)	39 (8.55%)	30 (7.25%)	60 (14.25%)	129 (9.99%)

Notes: The baseline survey was conducted between November 2nd and December 19th, 2015. The follow-up survey was conducted between April 6th and July 19th of 2016 with additional tracking efforts and surveys in October 2016 and February 2017.

Table 2: Summary statistics and randomization-balance at baseline

		Full sample	Control (C)	Treatment (A)		Treatment (B)	
	Obs.	Mean (SD)	Mean (SD)	Mean (SD)	Diff. from C [SE]	Mean (SD)	Diff. from C [SE]
Panel A: Household characteristics							
Household size	1,259	6.83 (3.77)	6.86 (3.52)	7.00 (4.25)	0.14 [0.33]	6.64 (3.52)	-0.22 [0.33]
No. of contributors	1,277	2.12 (1.98)	2.12 (2.01)	2.06 (1.95)	0.06 [0.14]	2.19 (1.98)	0.06 [0.12]
No. of children	1,273	4.17 (2.95)	4.29 (2.88)	4.29 (3.22)	0.00 [0.27]	3.94 (2.75)	-0.35 [0.24]
No. of rooms	1,284	3.23 (1.84)	3.37 (1.94)	3.14 (1.78)	-0.23 [0.17]	3.16 (1.79)	-0.21 [0.17]
Owns dwelling	1,291	0.74 (0.44)	0.78 (0.41)	0.70 (0.46)	-0.08* [0.05]	0.72 (0.45)	-0.56 [0.05]
Assets (z-score)	1,162	0.00 (1.00)	0.07 (1.07)	-0.06 (0.99)	-0.13 [0.13]	-0.01 (0.93)	-0.08 [0.11]
Tap water	1,291	0.46 (0.50)	0.40 (0.49)	0.50 (0.50)	0.10 [0.08]	0.47 (0.50)	0.07 [0.09]
Monthly income[+]	1,250	219,867 (327,192)	222,400 (337,538)	203,232 (283,402)	-19,168 [32,471]	233,565 (355,164)	11,165 [33,255]
Monthly consumption[+]	1,286	592,775 (408,015)	592,219 (402,390)	616,570 (448,328)	24,350 [49,884]	569,925 (370,276)	-22,294 [39,842]
Panel B: Respondent characteristics							
Female	1,265	0.80 (0.40)	0.80 (0.40)	0.79 (0.41)	-0.01 [0.05]	0.80 (0.40)	0.00 [0.06]
Age	1,277	36.23 (11.89)	37.72 (12.36)	35.38 (11.53)	-2.34* [1.18]	35.46 (11.59)	-2.26* [1.19]
Education	1,282	6.83 (3.69)	7.11 (3.66)	6.61 (3.71)	-0.49 [0.38]	6.74 (3.70)	-0.36 [0.35]
Literate	1,238	0.68 (0.47)	0.70 (0.46)	0.64 (0.48)	-0.06 [0.05]	0.68 (0.47)	-0.02 [0.04]
Econ. dependent	1,285	0.16 (0.36)	0.15 (0.36)	0.16 (0.37)	0.01 [0.03]	0.16 (0.37)	0.01 [0.03]
Receives aid	1,277	0.22 (0.41)	0.24 (0.43)	0.21 (0.41)	-0.03 [0.04]	0.21 (0.40)	-0.03 [0.03]
Married	1,291	0.62 (0.49)	0.59 (0.49)	0.60 (0.49)	0.01 [0.04]	0.66 (0.48)	0.07 [0.04]
Main contrib.	1,291	0.70 (0.46)	0.70 (0.46)	0.74 (0.44)	0.04 [0.04]	0.67 (.47)	-0.03 [0.04]
HH head	1,291	0.55 (0.50)	0.55 (0.50)	0.58 (0.49)	0.03 [0.05]	0.51 (0.50)	-0.04 [0.05]
Years as vendor	1,263	7.42 (7.46)	8.25 (8.08)	7.77 (7.41)	-0.48 [0.90]	6.18 (6.63)	-2.07** [0.86]
Sells nonfood items	1,291	0.14 (0.35)	0.14 (0.35)	0.14 (0.35)	0.00 [0.04]	0.14 (0.35)	0.00 [0.04]
Numeracy (z-score)	1,291	0.05 (0.97)	0.01 (0.97)	0.07 (0.94)	0.06 [0.08]	0.07 (0.98)	0.06 [0.09]

-continued-

Self-control (z-score)	1,273	0.00	-0.01	0.07	0.08	-0.05	-0.04
		(1.00)	(0.99)	(1.02)	[0.08]	(0.99)	[0.08]
Patience (z-score)	1,280	0.00	0.02	-0.07	-0.09	0.05	0.04
		(1.00)	(1.01)	(1.04)	[0.09]	(0.95)	[0.07]
Trust (z-score)	1,291	0.00	0.00	-0.04	-0.04	0.04	0.05
		(1.00)	(0.98)	(1.02)	[0.08]	(1.00)	[0.08]
Altruism (z-score)	1,267	0.00	-0.04	0.05	0.09	0.00	0.04
		(1.00)	(0.99)	(1.02)	[0.08]	(0.98)	[0.06]
Fatalist worldview	1,253	0.00	0.03	-0.02	-0.05	-0.01	-0.04
(z-score)		(1.00)	(0.99)	(1.03)	[0.08]	(0.98)	[0.10]
General risk attitude	1,262	3.61	3.53	3.67	0.14	3.66	0.13
(0-10)		(2.42)	(2.39)	(2.40)	[0.17]	(2.48)	[0.19]
Specific risk attitude	1,272	3.78	3.72	3.78	0.06	3.85	0.13
(0-10)		(2.52)	(2.53)	(2.46)	[0.16]	(2.57)	[0.19]
F-test of joint orthogonality (p-value)					0.27		0.31
Observations					914		914
Clusters					83		83

Notes: + indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99th percentile. Differences between treatment and control groups are estimates from OLS-regressions. Standard errors (clustered at the market-level) are reported in square brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Main treatment effects on financial literacy and behaviors

			Borrowing			Saving					
	(1) FL score (z)	(2) Budget index (z)	(3) Debt index (z)	(4) Any debt	(5) Total debt+	(6) Savings index (z)	(7) Any savings	(8) Total savings+	(9) Net savings+	(10) Investing+	(11) Services index (z)
Panel A: Unadjusted OLS results											
Treatment A	0.150*	0.063	-0.000	0.034	-16,589	0.156**	0.038*	175,566**	182,316**	150,300	0.085*
	(0.083)	(0.055)	(0.037)	(0.044)	(53,889)	(0.062)	(0.021)	(87,649)	(73,505)	(102,405)	(0.050)
Treatment B	0.081	0.018	0.005	-0.038	-3,397	0.038	0.026	14,049	3,099	49,982	0.117**
	(0.072)	(0.057)	(0.039)	(0.046)	(55,079)	(0.070)	(0.024)	(96,074)	(75,359)	(103,232)	(0.056)
Test: $A - B = 0$ (p-value)	0.39	0.43	0.91	0.13	0.82	0.14	0.61	0.15	0.04	0.43	0.60
Obs.	1,160	1,144	1,067	1,093	1,162	1,155	1,155	1,162	1,162	1,060	1,103
R^2	0.03	0.03	0.018	0.039	0.015	0.026	0.007	0.034	0.026	0.056	0.025
District FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Baseline controls	no	no	no	no	no	no	no	no	no	no	no
Panel B: Single difference with baseline controls											
Treatment A	0.111	0.085*	-0.025	-0.004	-19,204	0.122*	0.035	142,271*	169,601**	135,119 *	0.047
	(0.093)	(0.051)	(0.032)	(0.044)	(36,238)	(0.063)	(0.023)	(81,862)	(84,881)	(74,404)	(0.058)
Treatment B	0.065	0.029	-0.016	-0.098**	-12,393	0.012	0.018	-26,802	109.898	31,295	0.102*
	(0.083)	(0.048)	(0.035)	(0.042)	(36,444)	(0.061)	(0.026)	(77,574)	(77,724)	(81,327)	(0.058)
Test: $A - B = 0$ (p-value)	0.63	0.22	0.81	0.04	0.83	0.12	0.53	0.05	0.04	0.25	0.36
Obs.	912	885	750	836	914	903	903	914	914	770	747
R^2	0.090	0.105	0.120	0.117	0.264	0.243	0.067	0.317	0.234	0.360	0.147
District FEs	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Baseline controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean of y_t in control group	-0.061	0.010	-0.001	0.525	259,348	-0.037	0.87	519,384	386,323	384,136	0.009

Notes: + indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99th percentile. Standard errors, clustered at the market-level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Heterogeneous treatment effects regarding various subgroups

	(1) FL score (z)	(2) Budget index (z)	(3) Debt index (z)	(3) Savings index (z)	(4) Investing	(5) Services index (z)
Panel A: Male respondents						
Treatment A	0.049	0.062	0.021	0.126**	85,209	0.045
	(0.096)	(0.057)	(0.032)	(0.058)	(58,463)	(0.049)
Treatment B	0.043	-0.001	0.044	0.021	54,673	0.092
	(0.082)	(0.055)	(0.034)	(0.055)	(74,179)	(0.056)
Male	-0.097	0.153*	0.088*	0.206**	390,624 ***	0.190***
	(0.097)	(0.087)	(0.050)	(0.091)	(117,034)	(0.070)
Treatment A × Male	0.377**	-0.002	-0.177**	0.019	99,871	0.158
	(0.163)	(0.118)	(0.086)	(0.153)	(301,913)	(0.111)
Treatment B × Male	0.148	0.057	-0.276***	-0.033	-112,521	0.065
	(0.150)	(0.122)	(0.079)	(0.185)	(218,012)	(0.111)
Obs.	1,136	1,098	923	1,123	955	918
R^2	0.053	0.066	0.071	0.168	0.207	0.106
Panel B: Literate respondents						
Treatment A	-0.110	0.070	0.008	0.203**	170,572 **	0.057
	(0.110)	(0.077)	(0.059)	(0.092)	(82,323)	(0.083)
Treatment B	0.138	-0.028	-0.019	0.078	93,418.902	-0.087
	(0.121)	(0.080)	(0.060)	(0.081)	(61,856.548)	(0.099)
Literate	0.093	0.100	-0.018	0.177**	240,899 ***	0.109
	(0.090)	(0.067)	(0.060)	(0.084)	(79,911)	(0.080)
Treatment A × Literate	0.329***	-0.004	-0.042	-0.116	-87,129	0.021
	(0.123)	(0.095)	(0.069)	(0.119)	(138,301)	(0.105)
Treatment B × Literate	-0.134	0.062	-0.004	-0.088	-134,344	0.281**
	(0.138)	(0.098)	(0.078)	(0.119)	(120,169)	(0.116)
Obs.	1,115	1,078	909	1,102	935	904
R^2	0.063	0.070	0.060	0.164	0.187	0.101
Panel C: Relatively young respondents						
Treatment A	0.210**	0.060	-0.002	0.174**	123,135	0.098
	(0.096)	(0.065)	(0.049)	(0.072)	(121,120)	(0.061)
Treatment B	0.122	-0.030	-0.002	0.015	-2,412	0.111*
	(0.080)	(0.055)	(0.044)	(0.074)	(90,378)	(0.064)
Young	0.203**	0.019	0.074*	-0.030	-43,084	0.025
	(0.083)	(0.049)	(0.039)	(0.087)	(66,357)	(0.070)
Treatment A × Young	-0.267**	0.007	-0.037	-0.164	-40,469	-0.042
	(0.126)	(0.087)	(0.060)	(0.106)	(153,776)	(0.102)
Treatment B × Young	-0.163	0.137*	-0.037	-0.019	62,221	-0.021
	(0.123)	(0.082)	(0.058)	(0.108)	(134,592)	(0.110)
Obs.	1,160	1,121	942	1,147	975	937
R^2	0.055	0.064	0.058	0.165	0.176	0.078
District FEs	yes	yes	yes	yes	yes	yes
$y_{(t-1)}$ controls	yes	yes	yes	yes	yes	yes
Baseline controls	no	no	no	no	no	no

Notes: Standard errors, clustered at the market-level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Determinants of attrition

Baseline covariates	Binary for attrition	
	(1) Attrition	(2) Attrition
Treatment A	-0.030	0.085
	(0.023)	(0.099)
Treatment B	0.058*	0.388***
	(0.033)	(0.131)
Female	-0.053**	-0.012
	(0.024)	(0.027)
Age	-0.003***	-0.001
	(0.000)	(0.001)
Literate	-0.039*	0.014
	(0.020)	(0.028)
Monthly consumption+	-0.000**	-0.000
	(0.000)	(0.000)
Monthly income+	0.000	-0.000***
	(0.000)	(0.000)
Treatment A × Female		-0.053
		(0.041)
Treatment A × Age		-0.001
		(0.002)
Treatment A × Literate		-0.058
		(0.042)
Treatment A × Monthly consumption+		-0.000
		(0.000)
Treatment A × Monthly income+		0.000*
		(0.000)
Treatment B × Female		-0.075
		(0.070)
Treatment B × Age		-0.006***
		(0.002)
Treatment B × Literate		-0.105*
		(0.054)
Treatment B × Monthly consumption+		-0.000
		(0.000)
Treatment B × Monthly income+		0.000**
		(0.000)
Constant	0.311***	0.163**
	(0.0622)	(0.0746)
District dummies	Yes	Yes
R^2	0.05	0.07
n (Individuals)	1157	1157
n (Clusters)	83	83

Notes: + indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99th percentile. Standard errors, clustered at the market-level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Bounds estimates for treatments A and B

	(1) Worst case lower bound	(2) Unadjusted treatment effect	(3) Worst case upper bound
Panel A: Impact on the savings index (single difference)			
Treatment A	-0.037	0.121**	0.209***
	(0.059)	(0.055)	(0.066)
Treatment B	-0.196***	0.010	0.076
	(0.062)	(0.060)	(0.065)
Test: $A - B = 0$ (p-value)	0.02	0.09	0.06
Obs.	1,291	1,147	1,291
R^2	0.465	0.161	0.624
Panel B: Impact on financial literacy scores (single difference)			
Treatment A	-0.136	0.135*	0.396***
	(0.092)	(0.079)	(0.081)
Treatment B	-0.331***	0.080	0.468***
	(0.085)	(0.071)	(0.086)
Test: $A - B = 0$ (p-value)	0.04	0.49	0.45
Obs.	1,291	1,160	1,291
R^2	0.061	0.051	0.065
District dummies	yes	yes	yes
$y_{(t-1)}$ controls	yes	yes	yes

Notes: Standard errors, clustered at the market-level, in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

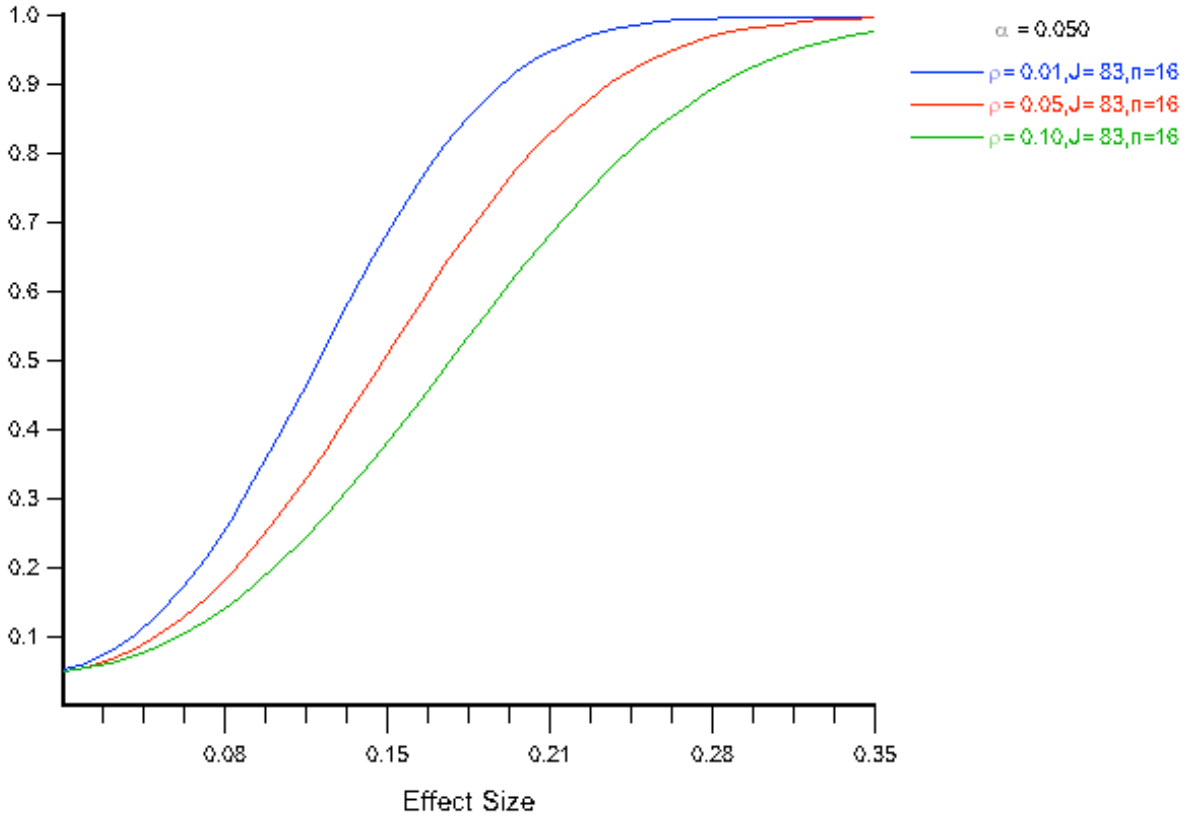

Figure 1: Impact of ICC on power (y) and minimum detectable effect size (x)

Appendix

(online appendix not intended for publication)

to accompany

"**Experimental evidence on the causal effects of financial education among small-scale retailers in rural Uganda**"

Appendix A: Measuring financial literacy

Appendix B: Baseline and follow-up questionnaires

Appendix A: Measuring financial literacy

Financial *literacy* is typically measured through a set of survey-items that measure financial *knowledge* (cf. Huston 2010, p. 303). In a second step, the scores on these dichotomous items (true/false) are summed up to generate a scale of financial knowledge to serve as a proxy for the latent trait "financial literacy". There are standard questions used for the assessment of financial literacy in developed economies (cf. Knoll and Houts 2012; Lusardi and Mitchell 2014, p. 10) and Cole et al. (2011) were the first to translate and adapt these questions to a developing economy context for their study of financial education and the demand for financial products in Indonesia. While these and similar survey items have been widely used in the literature on financial education in developing economies (e.g. Carpena et al. 2011; Sayinzoga et al. 2016), their psychometric properties have not been adequately assessed. This may be surprising since knowledge development is seen to be one of the primary goals of these types of financial education interventions (cf. Skimmyhorn 2016) and the need for a valid measure of financial literacy appears evident. Thus, I study the psychometric properties of these items and propose an alternative approach to generate valid financial literacy scores for individual respondents: Following Knoll and Houts' (2012) discussion of items used in the assessment of financial knowledge in large-scale household surveys, I use "item response theory" (IRT) to create a valid and reliable scale of financial literacy. IRT is a family of models widely used in educational and psychological measurement (see Rasch 1960 and Lord 1980 for key- contributions to this literature). A popular model used to design psychological measurement-scales is the two-parameter logistic model (2PLM) (cf. Birnbaum 1968). Here, the probability of an individual j to solve the item i is defined as

$$P(x_{ij} = 1 \,|\theta_j) = \frac{exp\{a_i(\theta_j - b_i)\}}{1+exp\{a_i(\theta_j - b_i)\}} \tag{1}$$

with

$$\theta_j \sim N(0,1) \tag{2}$$

where a_i and, b_i are the discrimination and difficulty parameters of item i respectively, and θ_j representing the latent trait (e.g. financial literacy) of individual j. Thus, the discrimination parameter a_i describes how well item i discriminates people of lower

and higher ability (θ_j), and b_i corresponds to the point on the latent scale (θ) where $P(x_i = 1 \,|\theta) = 0.5$ (i.e. the point on the latent scale where an individual has a greater possibility to score the item than indicated by chance). Since we assume θ to have a mean of zero by definition, an item i is relatively easy to solve if $b_i < 0$, and an item i is relatively hard to solve if $b_i > 0$. This model requires the assumption of local independence among items (solving an item must not be conditional upon solving another item) and θ to be unidimensional. While local independence is given by the design of the items and implementation into the survey instruments, I tested the assumption of unidimensionality through a factor analysis (principal factors). Indeed, only one factor is estimated with an eigenvalue > 1 and, thus, the assumption of a unidimensional θ appears to be met by the items included in the scale (cf. Figure A1 in Appendix A). To arrive at parameter estimates for a_i and, b_i, as well as to predict θ_j for all respondents in the dataset, I estimate equation (1) with five binary items that form the final financial literacy scale. Standard errors are clustered at the level of randomization (markets).

Table A1 shows the exact wording, discrimination and difficulty for the final set of five items. The items are ordered by their ability to discriminate in ascending order. Thus, item 1 is the least discriminating ($a_1 = 0.981$) and item 5 is the most discriminating ($a_5 = 1.629$). The difficulty ranges from -0.569 (item 2) to 0.463 (item 3). A graphic representation of these item characteristics is depicted in Figure A2 which shows the trace line for each item included in the scale. Item 3 is most difficult (furthest to the right) while item 2 appears to be easiest. Regarding the discrimination, it is obvious that the trace line for item 5 has the steepest slope while the slope of item 1 is most gradual. Another way to represent the features of each item is to plot the item information functions. Figure A3 shows the item information functions for each item. Figure A4 relates the latent trait back to the sumscores of items solved: Using the critical values of the z-distribution (-1.96 and 1.96) it appears that 95% of randomly selected individuals would solve between 0.451 and 4.51 items with a respondent of average ability (θ=0) scoring 2.56 (two or three) out of five items. Turning to the overall reliability of the scale, Figure A5 shows that the scale is most precise at the mean of θ with smallest standard errors close between -0.1 and 0.

Finally, I standardize the scale to have a mean of zero and a standard deviation of one for the control group: Figure A6 shows the full distribution of the estimated ability (θ for all individuals in our dataset at baseline.

References in Appendix A

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Table A1: Items and their psychometric properties of the final FL-scale

Item	Topic	Question and response options	a_i (SE)	b_i (SE)
1	Interest rate (loan)	Suppose you need to borrow 500,000 UGX. Two people offer you a loan. Which loan represents a better deal for you? A) One loan requires you to pay back 600,000 UGX in 1 month. **B) The second loan requires you to pay back in 1 month 500,000 UGX plus 15% interest.** Y) Don`t know Z) Refuse to Answer	0.981 (0.152)	0.274 (0.107)
2	Diversification	Is it riskier to plant…? A) multiple crops or **B) one crop** Y) Don`t know Z) Refuse to Answer	1.092 (0.166)	-0.569 (0.104)
3	Compound interest	Suppose you borrow 100,000 UGX at an interest rate of 2% per month, with no repayment for 3 months. After 3 months, do you owe A) less than. 102,000 UGX, B) exactly. 102,000 UGX, **C) or more than 102,000 UGX?** Y) Don`t know Z) Refuse to Answer	1.218 (0.146)	0.463 (0.087)
4	Interest rate (loan)	If you were offered a loan with 5% monthly interest rate and a loan with 20% annual interest rate, which loan would offer better value? A)5% monthly interest rate **B) 20% annual interest rate** Y) Don`t know Z) Refuse to Answer	1.346 (0.149)	0.130 (0.061)
5	Inflation	If you have UGX. 100,000 in a savings account earning 1% interest per annum, and prices for goods and services rise 2% over a 1-year period, can you buy A) more than, **B) less than,** C) or the same amount of goods in 1 year as you could today, with the money in the account?" Y) Don`t know Z) Refuse to Answer	1.692 (0.258)	-0.375 (0.085)

Notes: N=1,291. Results from fitting a 2PLM to the 5 items. Standard errors are clustered at the market-level. Items are coded to be binary. The correct response is coded to be equal to one. Wrong answers, missing values, and response options Y) and Z) are coded to be equal to zero.

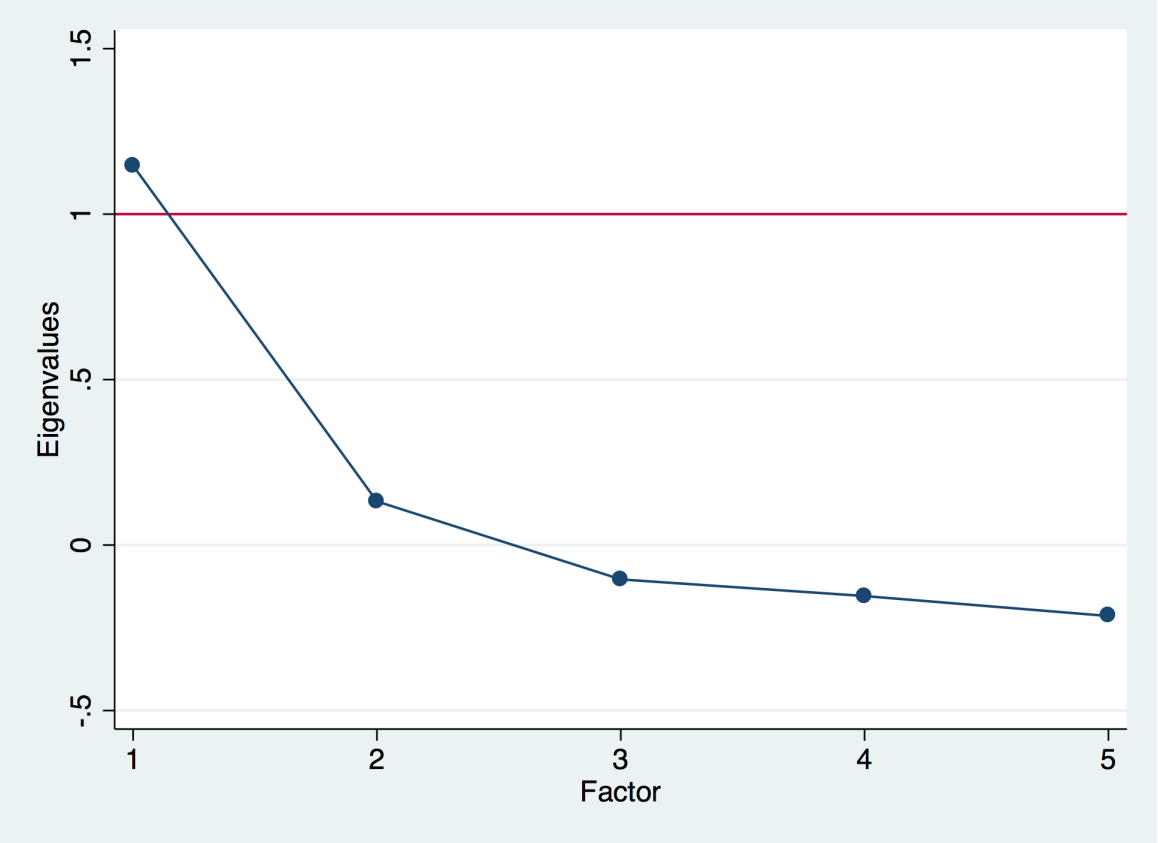


Figure A1: Screeplot of eigenvalues by factor after factor analysis (principal factors)

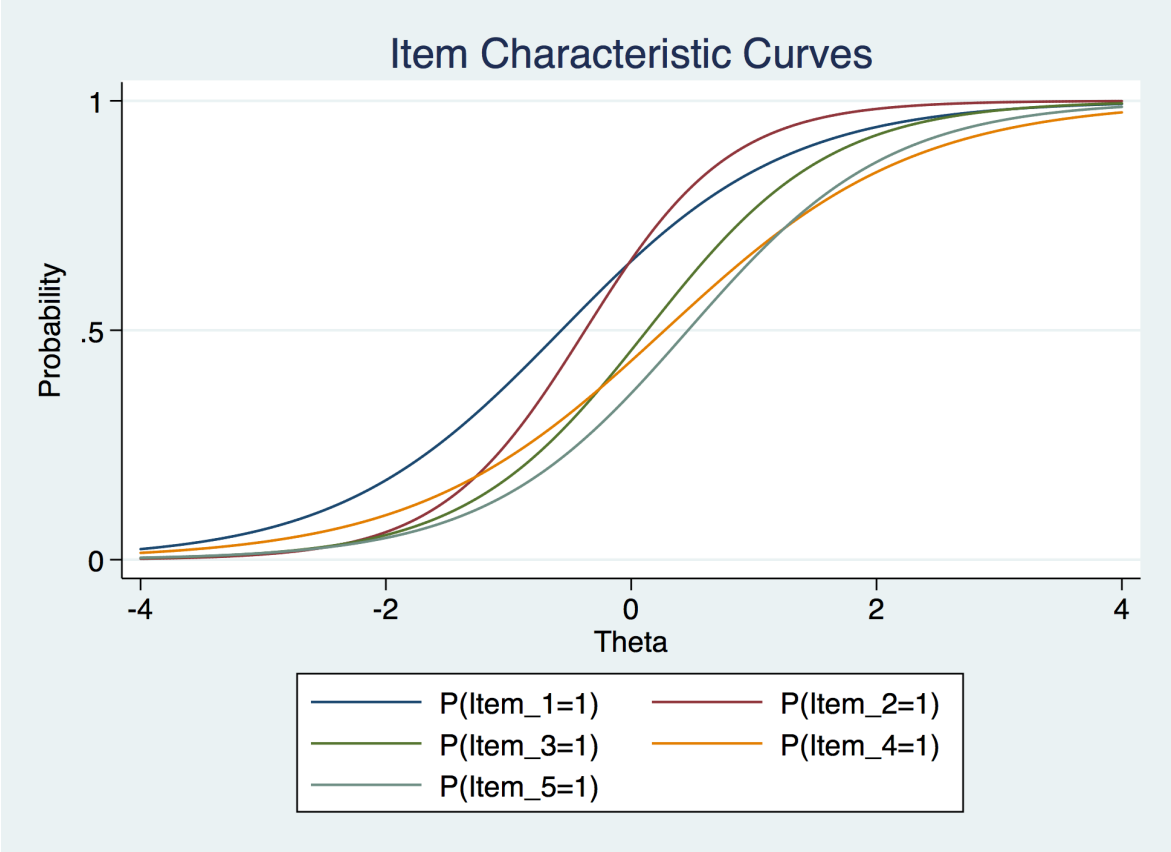


Figure A2: Item characteristic curves for the 2PLM financial literacy scale

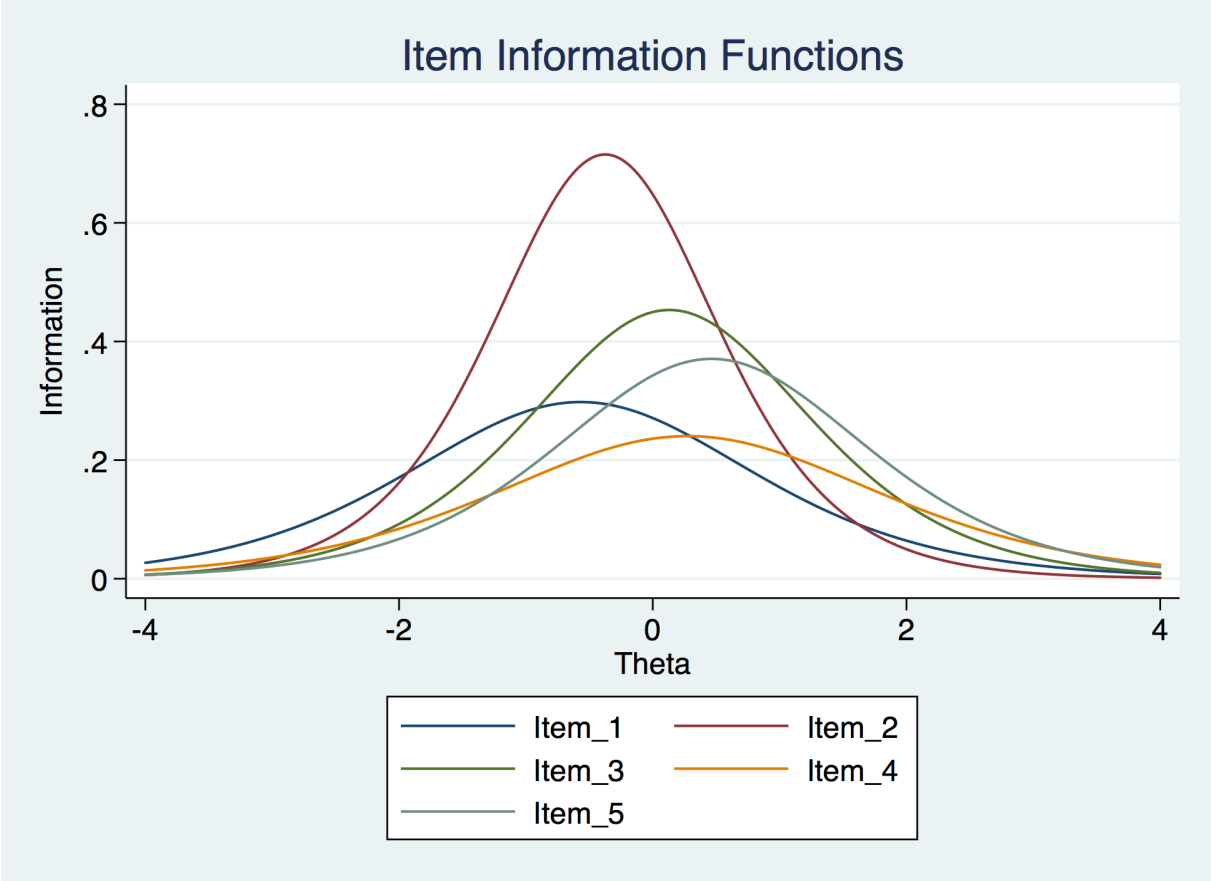


Figure A3: Item information functions for the 2PLM financial literacy scale

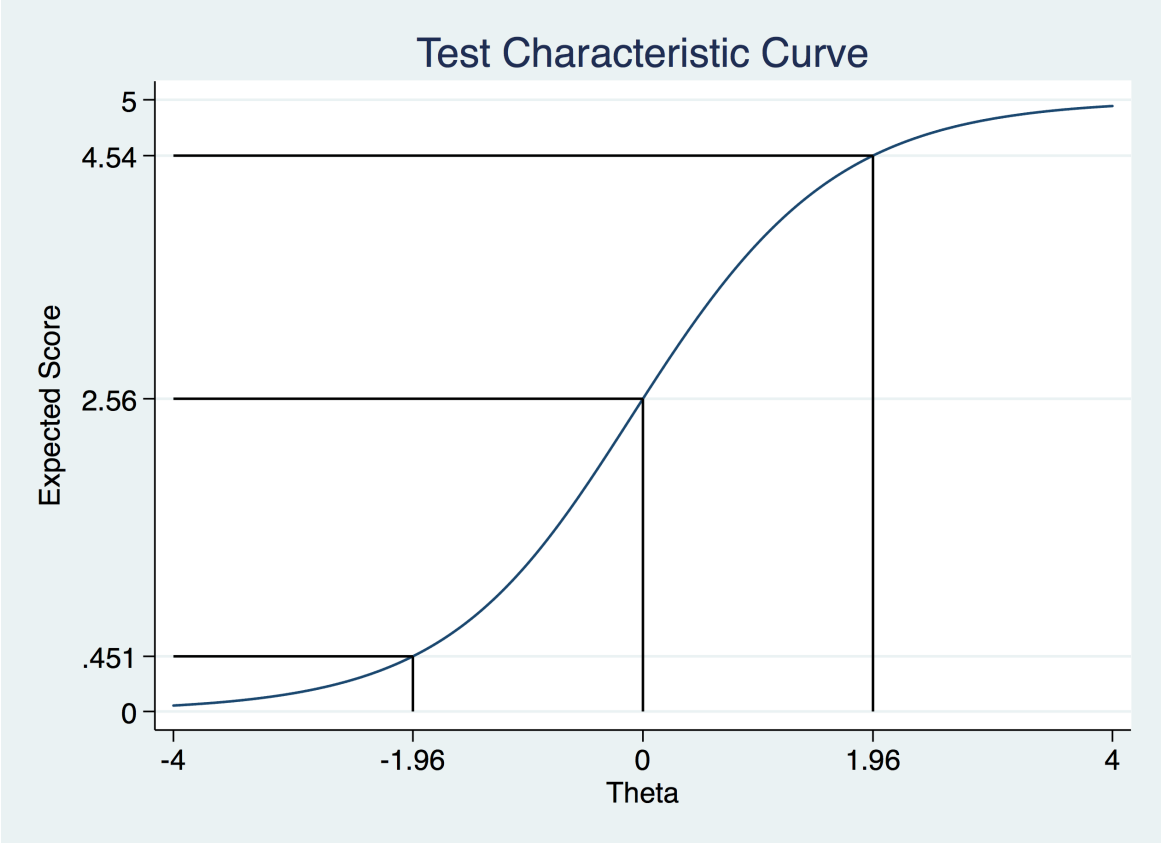


Figure A4: Test characteristic curve for the 2PLM financial literacy scale

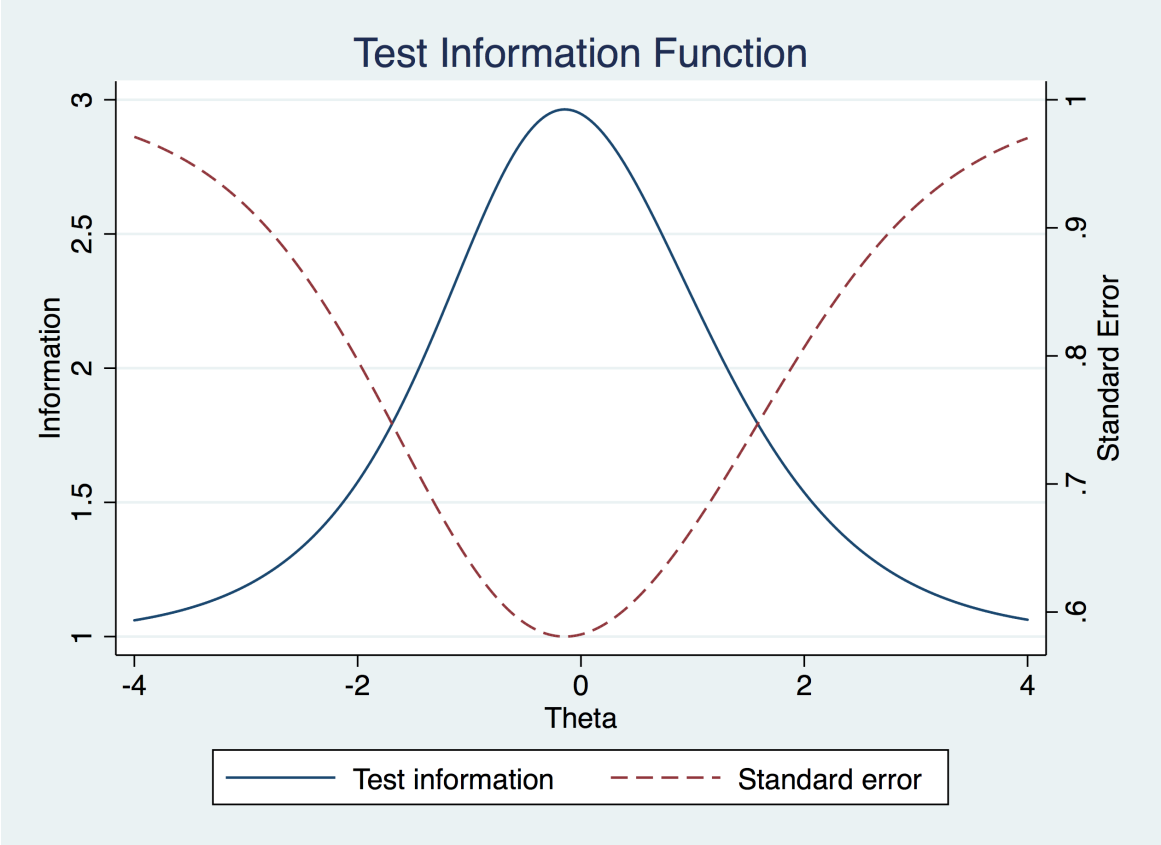


Figure A5: Test information function for the financial literacy scale

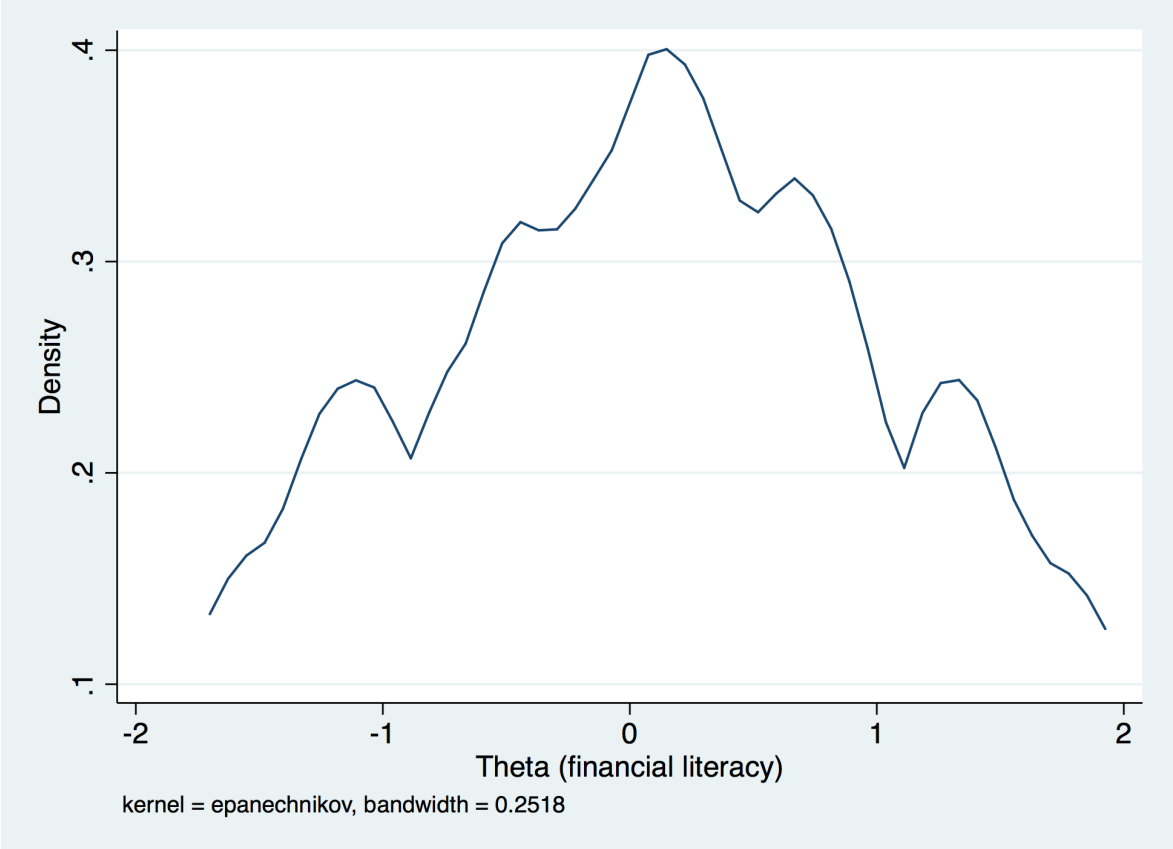


Figure A6: Distribution of standardized financial literacy IRT-scores at baseline

Appendix B: Baseline and follow-up questionnaires

FL-Impact Evaluation: Baseline Questionnaire (English)

Q1: (a) Date of Interview: DD/MM/YYYY: |___|___|/|___|___|/|___|___|___|___|		
(b) Time of Interview: HH:MM: |___|___|:|___|___|		
Q2: Market ID Code: _ _ _		
Q3: Respondent ID Code: _ _ (Rolling number)		
Q4: (a) Enumerator's details	Name:	ID Code: _____
(b) Supervisor's details	Name:	ID Code: _____
Q5: Location of interview:		
(a) District: _ _	(b) County: _ _	
(c) Sub-county: _ _ _ _ _ _ _	(d) Town: _ _ _ _ _ _ _ _ _ _ _ _ _ _ _ _	
(e) Parish: _ _ _ _ _ _ _	(f) Village/Neighborhood(88=N/A): _ _ _ _ _ _ _ _ _ _ _ _ _ _ _	
(g) Urban=1, Rural=2: _ _		
(h) Name of Market location: _ _ _ _ _ _ _ _ _ _ _ _		
Q6: ***Do not read:*** DOES THIS PERSON SEEM EMOTIONALLY AND MENTALLY CAPABLE OF COMPLETING THIS SURVEY?	1. Yes 2. No, intoxicated 3. No, mentally impaired 4. No, other: _____ ***If no, stop interview and refer the case to the survey supervisor***	
Q7: ***Do not read:*** IS THE INTERVIEW BEING CONDUCTED WITH THE INTERVIEWEE ALONE (EXCEPTING SURVEY STAFF)?	1. Yes 2. No → ***Politely ask to be allowed to interview the respondent alone. Stress that the interview is private and confidential.***	
Q8: ***Do not read:*** ARE YOU SUSPICIOUS THAT THE PERSON THAT YOU ARE INTERVIEWING IS NOT THE ONE WE SOUGHT FOR AN INTERVIEW?	0. No, not at all 1. A little suspicious → ***Explain*** 2. Very suspicious → ***Explain***	(b) WHY ARE YOU AT ALL SUSPICIOUS? _____

1 DEMOGRAPHICS				
Q9:	*GENDER*	MALE---------- 1 FEMALE---------- 2		
Q10:	What is your tribe? *ENTER THE CORRECT 2-DIGIT CODE IN THE SPACES AT FAR RIGHT. IF OTHER, WRITE TRIBE IN "OTHER" SPACE AND FILL 99 CODE*	01) BAAMBA 02) BABWISI 03) BAGANDA 04) BAKHONZO 05) BAKIGA 06) BASONGORA 07) BATORO 08) BATUKU	09) BANYAKOLE 10) BANYARWANDA 11) BANYOLE 12) BANYORO 13) CONGOLESE 14) IK (TEUSO) 15) ITESO 16) RWANDESE 99) OTHER: _____	|___|__|
Q11:	How old are you?	|___|_ |years		
Q12:	What is your religion? *PLEASE CIRCLE ONE*	01) Roman Catholic 02) Church of Uganda (Anglican) 03) Pentecostal 04) Muslim 99) Other		
Q13:	What is the highest level you have completed in school? *ENTER THE CORRECT 2-DIGIT CODE IN THE SPACES AT FAR RIGHT.*	01) none 02) P1 08) P7 14) S6 03) P2 09) S1 15) SOME TERTIARY INSTITUTE 04) P3 10) S2 16) COMPLETED TERTIARY INSTITUTE 05) P4 11) S3 17) SOME UNIVERSITY 06) P5 12) S4 18) COMPLETED UNIVERSITY 07) P6 13) S5 19) MASTER'S OR PHD		|___|__|
Q14:	Can you read or write in any language? *PLEASE CIRCLE ONE*	1) Neither able to read or write 2) Able to read only 3) Able to read and write		
Q15:	Have you ever received any Financial Literacy training?	YES----------1 NO ----------0		

	2 DEPENDENCY PROFILE MODULE	
Q16:	How would you describe yourself financially? I will read you four options, and you can tell me which is best.? By "dependent" I mean you receive more money in support from other people than you earn for yourself. *PLEASE MAKE SURE THE RESPONDENT TAKES A POSITION.*	Completely economically independent---------- 1 Mostly economically independent---------- 2 Mostly economically dependent---------- 3 Completely economically dependent---------- 4
Q17:	How many children do you support, including those children who are not biologically yours? By "children" I mean those who are less than 18 years old.	|___|___|children ***IF NONE (00)***
Q18:	How many other people who are not children do you support? These are people who are at least 18 years old. Remember by "support" I mean you regularly give them financial assistance that they do not have to work for.	|___|___|people ***IF NONE (00)***
Q19:	What is your marital status? *CIRCLE ONE*	01) Single 02) Living with Partner 03) Married 04) Seperated/Divorced 05) Widowed

3 HOUSEHOLD PROFILE MODULE

Read: Now I would like to know how many people are in your household. The household is defined as all of the people who normally live and eat their meals together in your home (this does not include visitors).

Q20:	How many people are in your household (including yourself)?	|___|___|people	
Q21:	Who do you usually stay with in your Household (excluding visitors)? *ENTER THE NUMBER(QUANTITY) OF EACH RELATION WHO THE RESPONDENT STAYS WITH. PROBE TO BE SURE YOU ARE ENTERING THE CORRECT NUMBER FOR EACH RELATION.* *ENTER "00" FOR ALL THOSE THAT DO NOT APPLY*	(a) OWN CHILDREN | _ _ | (b) BIOLOGICAL MOTHER | _ _ | (c) BIOLOGICAL FATHER | _ _ | (d) ADOPTIVE (LEGALLY) MOTHER | _ _ | (e) ADOPTIVE (LEGALLY) FATHER | _ _ | (f) STEP-MOTHER | _ _ | (g) STEP-FATHER | _ _ | (h) AUNT | _ _ | (i) UNCLE | _ _ | (j) OLDER SISTER | _ _ | (k) OLDER BROTHER | _ _ |	(l) YOUNGER SISTER | _ _ | (m) YOUNGER BROTHER | _ _ | (n) GRANDMOTHER | _ _ | (o) GRANDFATHER | _ _ | (p) SPOUSE | _ _ | (q) MOTHER IN LAW | _ _ | (r) FATHER IN LAW | _ _ | (s) OTHER FAMILY MEMBER | _ _ | (t) OTHER NON-FAMILY MEMBER | _ _ |
Q22:	So this means that including yourself, **XX** (***add up numbers and add the respondent himself***) people are staying at your place?	1. Yes, there are __ people staying in my household 2. No → Make corrections above and ask again	
Q23:	How many people regularly contribute some money to the household (including yourself)?	|___|___|people	

Q24:	Who contributes the most money to the household, in order to regularly meet the household's basic needs? *COMPLETE 24A IN COLUMN **A** ("CONTRIBUTER") OF THE TABLE AT THE RIGHT FOR THIS PERSON*	01) RESPONDENT HIM/HERSELF 02) BIOLOGICAL MOTHER 03) BIOLOGICAL FATHER 04) ADOPTIVE MOTHER 05) ADOPTIVE FATHER 06) STEP-MOTHER 07) STEP-FATHER 08) AUNT 09) UNCLE 10) OLDER SISTER 11) OLDER BROTHER 12) YOUNGER SISTER 13) YOUNGER BROTHER 14) GRANDMOTHER 15) GRANDFATHER 16) SPOUSE 99) OTHER	**A** **Contributer** 24A |___|	
Q25:	Who is the head of the household? *COMPLETE 25B IN COLUMN **B** ("HEAD") OF THE TABLE AT RIGHT FOR THIS PERSON. IF THIS IS THE SAME PERSON AS 24A, ENTER THE SAME INFORMATION.*	01) RESPONDENT HIM/HERSELF 02) BIOLOGICAL MOTHER 03) BIOLOGICAL FATHER 04) ADOPTIVE MOTHER 05) ADOPTIVE FATHER 06) STEP-MOTHER 07) STEP-FATHER 08) AUNT 09) UNCLE 10) OLDER SISTER 11) OLDER BROTHER 12) YOUNGER SISTER 13) YOUNGER BROTHER 14) GRANDMOTHER 15) GRANDFATHER 16) SPOUSE 99) OTHER		**B** **Head** 25B |___|
Q26:	What is the highest level of education [*READ RESPONSE TO 24A, THEN REPEAT FOR 25B*] has reached? *ENTER THE CODE AT RIGHT FOR EACH PERSON.* *CHECK WITH ABOVE TO ENSURE YOU FILL THE CORRECT INFORMATION FOR THE CORRECT PRESON*	01) NONE 02) SOME PRIMARY 03) COMPLETED PRIMARY 04) SOME SECONDARY 05) COMPLETED SECONDARY (S6) 06) SOME TERTIARY INSTITUTE 07) COMPLETED TERTIARY INSTITUTE 08) SOME UNIVERSITY 09) COMPLETED UNIVERSITY 10) MASTER'S OR PHD 98) DON'T KNOW	**26A** |___|___|	**26B** |___|___|
Q27:	What is the **MAIN** way [*READ RESPONSE TO 24A (Contributer), THEN REPEAT FOR 25B (Head)*] gets money? *ENTER THE CORRECT 2-DIGIT CODE AT RIGHT FOR EACH PERSON.* *CHECK WITH ABOVE TO ENSURE YOU FILL THE CORRECT INFORMATION FOR THE CORRECT PRESON*	01) VENDING OF FOOD OR ITEMS AT THE MARKET 02) SUBSISTENCE FARMER/AGRICULTURE 03) COMMERCIAL FARMER/AGRICULTURE 04) MAKING BRICKS FOR SALE 05) MAKING CHARCOAL FOR SALE 06) COLLECTING FIREWOOD OR GRASS FOR SALE 07) DIGGING IN SOMEONE ELSE'S GARDEN 08) TAKING CARE OF SOMEONE ELSE'S ANIMALS 09) TAKING CARE OF OWN ANIMALS 10) BREWING ALCOHOL/BEER 11) MONEY-LENDING 12) BODA-BODA/TAXI DRIVING 13) FISHING 14) QUARRYING 15) SMALL-SCALE VOCATION (EX. METAL-WORK, CARPENTRY, SHOE-REPAIR, SEWING) 16) SALOON (CUTTING, PLAITING HAIR) 17) HEALTH OR NGO WORKER 18) SOLDIER/POLICEMAN 19) TEACHER OR OTHER PUBLIC SERVANT 20) POLITICAL POSITION 21) WORK IN ANOTHER PERSON'S HOME (EX. ASCARI, MAID) 22) RENTING LAND 23) SMALL-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH LESS THAN 100,000 UGX 24) LARGER-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH MORE THAN 100,000UGX 25) NON-SALARIED (WAGE-EARNING FOR HOURS WORKED) EMPLOYEE IN CHURCH 26) SALARIED EMPLOYEE IN CHURCH 27) OTHER WAGE EMPLOYMENT (CASUAL LABOR – MONEY EARNED FOR HOURS WORKED OR JOB COMPLETED) 28) SALARIED EMPLOYEE IN A COMPANY OR FIRM 29) OTHER SMALL BUSINESS OWNER – BUSINESS ASSETS VALUED AS LESS THAN 100,000 UGX 30) OTHER LARGER BUSINESS OWNER – BUSINESS ASSETS VALUED AS MORE THAN 100,000 UGX 31) TRANSFERS FROM OTHER PEOPLE 98) DON'T KNOW 99) OTHER: [SPECIFY IN RESPONSE SPACE] _____	**27A** |___|___|	**27B** |___|___|

Read: Now I want to ask you about your household's dwelling(s). Remember, by household I mean the people who sleep here most nights and share the meals with you (this does not include visitors).

Q28: How many rooms / huts are there for sleeping?		_ _ rooms/hut
Q29: What type of material is used for the walls of the dwelling where you sleep? ***Circle all that apply.***	1. Burnt/stabilized bricks with cement 2. Burnt/stabilized bricks with mud 3. Cement Blocks 4. Concrete 5. Stone with cement	6. Unburnt Bricks with cement 7. Unburnt Bricks with mud 8. Wood 9. Mud and poles 99. Other
Q30: Is this dwelling rented, subsidized, provided free to you, or owned by your household? ***Circle one!***	1. Owned 2. Free (from government or NGO) 3. Free (from a private citizen) 4. Subsidized (from government or NGO) 5. Subsidized (from a private citizen)	6. Rented (from government or NGO) 7. Rented (from a private citizen) 8. Squatting (public land) 9. Squatting (private land) 99. Other
Q31: What is your household's main source of fuel or energy for lighting? ***Circle all that apply.***	1. Electricity 2. Gas 3. Paraffin (Lantern) 4. Paraffin (Tadooba)	5. Candle 6. Firewood 7. Cow dung or grass (reeds) 8. Solar 99. Other
Q32: What is the main source of drinking water that your household uses? ***Prompt if necessary. Circle one.***	1. TAP WATER FROM PIPE 2. PRIVATE BOREHOLE 3. SHARED BOREHOLE 4. RAIN WATER 5. SACHET	6. OTHER FILTERED WATER 7. WELL 8. STREAM/RIVER 9. DAM 99. OTHER
Q33: How many plots of land does your household own? *PLEASE ONLY LIST PLOTS THAT YOU YOURSELF OR YOUR HOUSEHOLD MEMBERS OWN. REMEMBER, THE HOUSEHOLD MEMBERS ARE THE PEOPLE THAT NORMALLY LIVE AND EAT THEIR MEALS TOGETHER IN YOUR HOME.*	[____] number of plots	

Q34: Read: Now I want to ask you about the items owned by you and your household. I want to remind you that the purpose of this survey is not to provide assistance, so please respond fully and completely, as your answers will not affect any kind of benefits.

How many of each of the following items do you and your household own? ***Read each asset off list and write number***

|__|__| a) Donkeys	|__|__| p) Motor vehicle (car or truck)	|__|__| ae) Car batteries
|__|__| b) Oxen	|__|__| q) Mobile phones	|__|__| af) Generator
|__|__| c) Cattle (excluding oxen)	|__|__| r) Beds	|__|__| ag) Sewing machines
|__|__| d) Goats	|__|__| s) Sofas	|__|__| ah) Boat or canoe
|__|__| e) Sheep	|__|__| t) Chairs	|__|__| ai) Radios
|__|__| f) Pigs	|__|__| u) Water-heaters	|__|__| aj) Cassette or CD players
|__|__| g) Chickens and Turkey	|__|__| v) Tables	|__|__| ak) Televisions
|__|__| h) Ducks and Guinea fowls	|__|__| w) Stools	|__|__| al) Video cassette or DVD player
|__|__| i) Doves and pigeons	|__|__| x) Mattresses	|__|__| am) Laptop or desktop computer
|__|__| j) Hoes	|__|__| y) Blankets	|__|__| an) Wheelbarrow
|__|__| k) Pangas	|__|__| z) Kettle	|__|__| ao) Speakers
|__|__| l) Granary	|__|__| aa) Iron	|__|__| ap) Helmets
|__|__| m) Bicycles	|__|__| ab) Jerry cans	|__|__| aq) Mirrors
|__|__| n) Motorcycles	|__|__| ac) Pots and pans	|__|__| ar) Watches
|__|__| o) Stoves	|__|__| ad) Fans	|__|__| as) Other ***(Specify in Q35 –for items that they feel are very important)***

Q35: If "Other" describe:

Q36: In general, how would you describe your own present living conditions?	1. Very Bad 2. Fairly bad 3. Neither good nor bad	4. Fairly good 5. Very good
Q37: Looking back, how do you rate your living conditions compared to 12 months ago?	1. Much Worse 2. Worse 3. Same	4. Better 5. Much Better

Q38: Looking ahead, do you expect your living conditions to be better or worse in 12 months time?	1. Much Worse 2. Worse 3. Same	4. Better 5. Much Better

4 ECONOMIC ACTIVITIES MODULE

Now I want to ask you about what work **<u>you</u>** did to earn money during the last 90 days. Please take a moment to think about what work you did to earn money in that time. Please tell me the activities that you got money from in these months.

*FOR EACH OF THE ACTIVITIES, COMPLETE THE TABLE BELOW FOR **QUESTIONS 39-42***

01) VENDING OF FOOD OR ITEMS AT THE MARKET
02) SUBSISTENCE FARMER/AGRICULTURE
03) COMMERCIAL FARMER/AGRICULTURE
04) MAKING BRICKS FOR SALE
05) MAKING CHARCOAL FOR SALE
06) COLLECTING FIREWOOD OR GRASS FOR SALE
07) DIGGING IN SOMEONE ELSE'S GARDEN
08) TAKING CARE OF SOMEONE ELSE'S ANIMALS
09) TAKING CARE OF OWN ANIMALS
10) BREWING ALCOHOL/BEER
10) MONEY-LENDING
11) BODA-BODA/TAXI DRIVING 11) SMALL-SCALE VOCATION (EX. METAL-WORK, CARPENTRY, SHOE-REPAIR, SEWING)
12) FISHING
13) QUARRYING
14) SMALL-SCALE VOCATION (EX. METAL-WORK, CARPENTRY, SHOE-REPAIR, SEWING)
15) SALOON (CUTTING OR PLAITING HAIR)
16) HEALTH OR NGO WORKER
17) SOLDIER/POLICEMAN
18) TEACHER OR OTHER PUBLIC EMPLOYEE
19) POLITICAL POSITION
20) WORK IN ANOTHER PERSON'S HOME (EX. ASCARI, MAID)
21) WORK IN OWN HOME
22) RENTING LAND
23) SMALL-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH LESS THAN 100,000 UGX
24) LARGER-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH MORE THAN 100,000 UGX
25) NON-SALARIED (WAGE-EARNING FOR HOURS WORKED) EMPLOYEE IN CHURCH 18) SALARIED EMPLOYEE IN CHURCH
26) SALARIED EMPLOYEE IN CHURCH
27) OTHER WAGE EMPLOYMENT (CASUAL LABOR – MONEY EARNED FOR HOURS WORKED OR JOB COMPLETED)
28) SALARIED EMPLOYEE IN A COMPANY OR FIRM
29) OTHER SMALL BUSINESS OWNER – BUSINESS ASSETS VALUED AS LESS THAN 100,000 UGX
30) OTHER LARGER BUSINESS OWNER – BUSINESS ASSETS VALUED AS MORE THAN 100,000 UGX
98) DON'T KNOW
99) OTHER: [SPECIFY IN RESPONSE SPACE]

	Q39:	**Q40:**	**Q41:**	**Q42:**
	ACTIVITY CODE FROM ABOVE	How much money did you get for [*ACTIVITY*] in the last 90 days? It's okay to estimate. *DON'T KNOW = 98* *RESPONSE IN UGX*	In which months did you do [*ACTIVITY*]? A) August B) September C) October D) November *CIRCLE ALL THAT APPLY*	Out of the past 90 days, during how many days did you spend some time doing [*ACTIVITY*]? It is okay to estimate *HELP RESPONDENT ESTIMATE*
1	|__|__|		A B C D	|__|__| days
2	|__|__|		A B C D	|__|__| days
3	|__|__|		A B C D	|__|__| days
4	|__|__|		A B C D	|__|__| days
5	|__|__|		A B C D	|__|__| days

Q43: Do you keep a log or record of expenses and revenues for any of your businesses?	1. Yes 0. No
Q44: Have you formally registered any of your businesses with regulatory authorities?	1. Yes 0. No
Q45: Do you pay any business taxes?	1. Yes 0. No
Q46: What is your main income-generating activity? ***[Record Activity using code from above]***	
Q47: How satisfied are you with your main income generating activity ***[say activity from Q46]?***	1. Very satisfied 2. Somewhat satisfied 3. Somewhat unsatisfied 4. Very unsatisfied

Q48:	How much money did you get in total in the last 3 months that you did not work for? *PROBE - ENCOURAGE RESPONDENT TO ESTIMATE DON'T KNOW = -9,999,998*	|___|,|___|___|___|,|___|___|___|UGX

Read: Now I would like to ask you some questions related to your vending activities in the market. *SKIP TO 59 IF PERSON IS NOT A MARKET VENDOR*

Q49: For how many years have you been vending food, vegetables or other items on markets?	__ __ years
Q50: For how many years have you been vending in this particular market?	__ __ years
Q51: Are you vending regularly on this market?	1) Yes, whenever the market takes place. 2) Whenever I have something to sell. 3) I only rarely attend this market as a vendor.
Q52: Are any of your close friends, neighbors and household members also market vendors?	1. Yes 0. No → **to Q55**
Q53: If yes, on which market?	1. On this market → **Q 55** 2. On other markets
Q54: If on other markets, please indicate where:	Note here using the market IDs from the separate list:
Q55: Are you also vending items in another market?	1) Yes, more than once a week. 2) Yes, once a week. 3) Yes, once a month or more (but less often than once a week) 4) Yes, but less than once a month 5) No → ***go to Q57***
Q56: Please tell me the location of the markets on which you are vending most often. *USE MARKET IDs FROM SEPARATE LIST!*	1) Location of market attended most often as a vendor _____ 2) Location of market attended second most often as a vendor _____ 3) Location of market attended third most often as a vendor _____
Q57: What type of items are you selling on the market?	1) Fresh food items 2) Cooked food items 3) Non-food items 4) Livestock 99) Other (Specify): _____
Q58: From your house, how long does it take you to get to this market?	1) Less than 30 minutes by boda/motor-vehicle. 2) Less than 30 minutes by boda/motor-vehicle but more than 30 minutes on foot. 3) More than 30 minutes by motor-vehicle. 98) Don`t know

5 CONSUMPTION AND EXPENDITURES			
Now I am going to ask you some questions about the consumption of your household. First, for each one of the following food items, we want to know about consumption of the item that took place within your household. We are also not asking about expenditure, but rather we want you to value the actual amount that you and your household consumed in the past 7 days. For example, imagine that your household purchased a bag of rice 7 days ago for 4000 USH. If your household has not yet consumed any of it, then the consumption value of this rice would be 0. If your household has consumed half of the bag, then the consumption value would be 2000 USH. Your consumption calculation should also include items that were produced at home, or given to your household as gifts.			
	Q59:	**Q60:**	**Q61:**
	In the **past 7 days**, did members of the household eat/drink [item] within the household?	How many days did the household consume [item] in the **past 7 days**?	If you had to buy the exact amount of [item] consumed by your household in the **past 7 days**, how much would it cost?
	1. Yes 0. No	__ __ days	_ _ _ _ _ _USH
a) Cassava			
b) Potatoes (sweet, irish, yams)			
c) Rice			
d) Meat and chicken (beef, pork, goat, bush meat)			
e) Fish			
f) Eggs			
g) Posho			
h) Matooke			
i) Millet bread			
j) Palm Oil			
k) Sorghum flour			
l) Sliced bread and buns			
m) Beans and peas			
n) Porridge/Bushera			
o) Milk			
p) Fruits (bananas, apples, oranges, mangos, jackfruit, pineapple, etc.)			
q) Vegetables (tomatoes, onions, cabbage, dodo, avocado, popo etc.)			
r) Infant formula foods			
s) Oil/butter			
t) Sodas			
u) Ground nuts (in shell, pounded, pasted)			
v) Salt			
w) Sugar			
x) Tea			
y) Coffee			
z) Alcohol			

Read: Now, I would like to know about outside consumption of food and drink. That is, I would like you to estimate, to the best of your knowledge, the value of the household's consumption of the following goods that took place outside of the household. Please include gifts as well as purchases.			
	Q62:	**Q63:**	**Q64:**
	Within the **past 7 days**, did the members of this household take any [item] outside the household?	How many days did the household or any member spend on [item] in the **past 7 days**?	What was the total value of [item] eaten/drunk outside of the household in the **past 7 days**?
	1. Yes 0. No	_ _ days	_ _ _ _ _ USH
a) Full meals (breakfast, lunch, or dinner)			

b) Snacks (chapati, chips, cassava, samosas etc..)			
c) Muchomo (chicken, goat, pork, beef)			
d) Sodas and juices			
e) Arege (local liquor)			
f) Kwete (local beer)			
g) Wine, commercial beer and liquor			

Q65: Now I will read you a list of items. For each item or expense, please tell me how much your household spent on the item in the **past 4 weeks**.

Item	*USH*
a) Charcoal/coal	
b) Firewood	
c) Kerosene/paraffin	
d) Other fuel	
e) Matches, lighters, candles, lamp/stove wicks	
f) Laundry soap, toilet soap	
g) Cigarettes or tobacco	
h) Airtime, internet, and phone-charging	
i) Public transportation – bus, taxi fares, petrol, boda boda	
j) Newspapers or magazines	
k) Batteries	
l) Personal care – toilet paper, toothpaste, hair products, razor, perfumes, lotions, make-up, beauty salons (exclude toilet/laundry soap)	
m) Sports, theaters, and other forms of entertainment	
n) Expenses in hotels and other forms of lodging	
o) Sports betting and other forms of gambling	

Q66: Now I will read you another list of items. For each item or expense, please tell me how much your household spent on the item **in the past 12 months**.

Item	*USH*
a) Clothes/shoes/material for adult males over the age of 18	
b) Clothes/shoes/material for adult females over the age of 18	
c) Clothes/shoes/material for boys under the age of 18 (excluding school uniforms)	
d) Clothes/shoes/material for girls under the age of 18 (excluding school uniforms)	
e) Modern medical treatment and medicines	
f) Traditional medical treatment and medicines	
g) School fees	
h) Other educational expenses (exercise books, pens, pencils, uniforms, maintenance, club fees, etc.)	
i) Cooking items/utensils, such as pots (except coal pot), pans, buckets, mortar, pepper grinder, grater, strainer, etc.	
j) Donations to the church or mosque	
k) Electricity	
l) Water charges	
m) Home improvements	
n) Club membership fees (unions, Rotary, social clubs, traditional groupings)	
o) Payments to domestic servants (security guards, cleaner, laundry person)	

	Read: *Thank you. Now I would like to ask you some general questions on how money issues are handled in your household. Please remember that your answers are strictly confidential and only used for research purposes.*

Q67:	If someone has some money but wants to make sure he does not spend it, what is the best way for him to put it? *CIRCLE ONE*	POCKET---------- 01 TIN WHERE HE/SHE STAYS---------- 02 HIDDEN AT HOME STAY (EX. IN MATTRESS) ---------- 03 IN A HOLE IN GARDEN---------- 04 ROTATING SAVINGS CLUB (ROSCA) ---------- 05 SACCO---------- 06 GROUP ACCOUNT AT A FORMAL BANK---------- 07 INDIVIDUAL ACCOUNT AT A FORMAL BANK---------- 08 HAVE ANOTHER PERSON (E.G. A FRIEND, MY MOTHER) HOLD IT FOR HIM/HER---------- 09 BUY THINGS THAT HE/SHE CAN SELL IF HE NEEDS TO (SUCH AS A GOAT OR A BICYCLE) ----------10
Q68:	Imagine you had to move to somewhere that would take you 30 minutes to walk to. You are not in a hurry. Would you pay for transport (by boda-boda or taxi) **or** would you walk for free? *CIRCLE ONE*	DEFINITELY PAY FOR TRANSPORT---------- 1 PROBABLY PAY---------- 2 PROBABLY WALK FOR FREE ---------- 3 DEFINITELY WALK---------- 4
Q69:	How often does it happen that you fear that you might not be able to get enough food to eat? Is it often, sometimes, rarely or never? *CIRCLE ONE*	Often---------- 1 Sometimes ---------- 2 Rarely---------- 3 Never---------- 4
Q70:	Are you involved in making decisions about how to use money in your household? *READ ALL RESPONSE OPTIONS – CIRCLE ONE*	Yes, you make all financial decisions alone---------- 1 Yes, you are involved in all financial decisions in the household---------- ---2 Yes, you are involved in SOME financial decisions, but not all-----3 No, you are not involved in financial decisions---------- 4

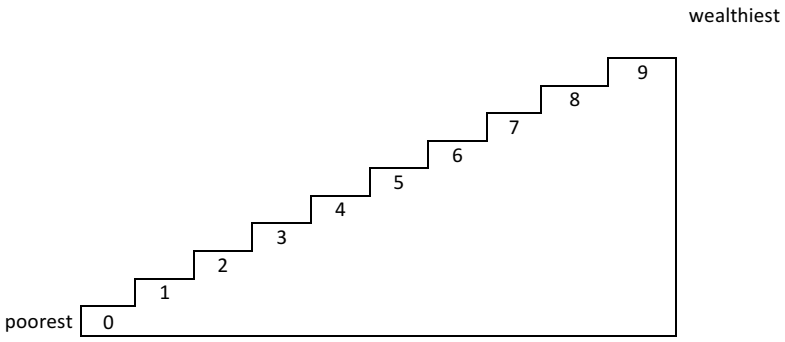
6 COMMUNITY AND SOCIAL NETWORKS MODULE

Read: Now I would like to ask you some questions about your relative standing within the community.

Enumerator Instruction: Show ladder card.

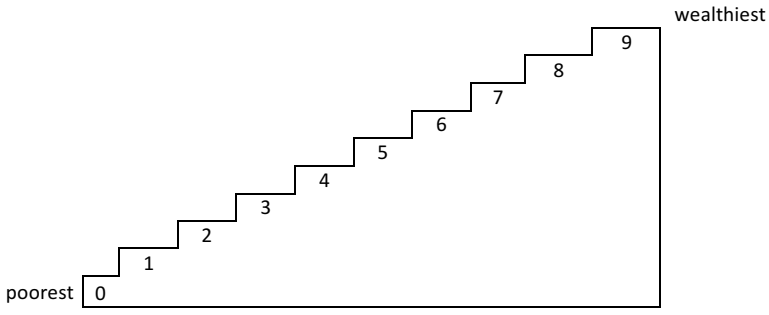
Q71: Imagine a 9-step ladder where the poorest people in the community stand on the lowest step and the wealthiest stand on the highest step. Where do you see yourself on the wealth ladder?

CIRCLE ONE RESPONSE



Q72: Think about all of your plans for the near future. If these plans are successful, in 5 years time, where would you see yourself on the wealth ladder?

CIRCLE ONE RESPONSE



Read: Now I want to talk about relationships in the community. I know that it is common that when someone is in need, he can go to other people in the community to ask for help, and sometimes in exchange this person can help him in return.

Q73: If you find yourself in need, for example to pay for school fees or funeral expenses or if you need a job, is there a relative or family member who you can go to?	1. Yes 0. No	
Q74: If you find yourself in need, for example to pay for school fees or funeral expenses or if you need a job, is there a local politician in the community who you can go to?	1. Yes 0. No	
Read: Thank you. Now I would like to ask you about any groups in which you are involved. These include formal groups that have meetings, but also informal groups where you happen to meet with someone unannounced.		
Q75: How many community groups are you a currently a member of, participate in, interact with, or volunteer for?	__ __ groups If ***zero***, go to **Q 81**	
Read: Now think about the group you are meeting with most often		
Q76: How often do you come together to work with this group?	1. Daily 2. Once or twice a week 3. Once or twice a month 4. A few times a year	5. Once a year 6. Less than once a year 7. Never
Q77: Does your group do income generating activities to make money for the whole group or for individual group members?	1. Often 2. Sometimes	3. Rarely 4. Never
Q78: Are financial matters an important topic in your group meetings and activities?	1. Often 2. Sometimes	3. Rarely 4. Never
Q79: How many times in the last 30 days have you discussed about money as a group in a meeting?	[_____] times *DON'T KNOW: -98*	
Q80: How many times in the last 30 days have you discussed personal money issues with another person who is in your group? This does not necessarily have to be for a group event, it could just be asking advice from a fellow group member about money issues.	[_____] times *DON'T KNOW: -98*	

	7 BUDGETING MODULE	
Q81:	Do you regularly keep track of how much money you spend?	YES---------- 1 NO ---------- 2 → ***TO Q 87***
Q82:	How do you usually keep track of how much money you spend? *READ ALL RESPONSE OPTIONS - CIRCLE ONE!*	Write it down ---------- 1 Someone else writes it down for you ---------- 2 Make a mental plan ---------- 3 Other: |_____|---------- 9
Q83:	How many times in the last 6 months have you done this activity in order to keep track of the money you spend? *HELP THE RESPONDENT ESTIMATE*	|__|__|times
Q84:	In the last 6 months, how many times has your plan failed, either because you got less money than you expected or because you had to spend more money than you expected?	|__|__|times ***IF 0 → TO Q86***
Q85:	What did you do when your plan failed? *READ ALL RESPONSE OPTIONS* *CHOOSE ONE!*	Borrowed money ---------- 1 Sold something that you owned ---------- 2 Found some other work to do in order to make more money ---------- 3 Spent less on the things that you did not need as much ---------- 4 Did nothing ---------- 5 Other |_____|----- 9
Q86:	Do you keep separate records for your business inflows and outflows and your personal finances?	1. Yes 0. No

Q87:	Which of the following are included in a budget? READ OUT ALL OPTIONS! *CIRCLE ALL THAT THE RESPONDENT THINK IS INCLUDED*	A) Expected income B) Last month's income C) Expected expenditure D) Last month's expenditure E) Planned savings F) I'm not sure

8 PAYMENTS MODULE			
Q88:	Have you ever made a payment using your mobile phone?	1 *yes* 2 *no → **Q92***	
Q89:	How often do you make payments using your mobile phone?	a) ___ *times*	b) *per* _____ *(time unit)*
Q90:	Why do you make payments using you mobile phone? *CIRCLE ONE*	1 convenience 2 safety 3 required by partner/ receiver 4 all of above 99 Other	
Q91:	In which context do you use your phone to make payments? *CIRCLE ONE*	1 private / family 2 business 3 Both 99 Other	
9 INSURANCE MODULE			
Q92:	Have you ever purchased an insurance product?	1 *Yes* 2 *No → **Q 94***	
Q93:	Which kind of insurance product have you purchased?	_____	
Q94:	Are you currently covered by any kind of insurance?	1 *Yes* 2 *No → **Q 96***	
Q95:	If yes, which insurance?		

10 FINANCIAL LITERACY MODULE

ENUMERATOR, PLEASE READ OUT THE QUESTIONS AS THEY ARE WRITTEN AND CIRCLE THE ANSWER GIVEN BY THE PARTICIPANT. DO NOT ASSIST THE PARTICIPANT IN ANSWERING THE QUESTIONS AND DO NOT EXPLAIN WHAT THE CONCEPTS REFERED TO IN THE QUESTIONS MEAN.

Q96: Suppose you borrow 100,000 UGX at an interest rate of 2% per month, with no repayment for 3 months. After 3 months, do you owe

A) less than. 102,000 UGX,
B) exactly. 102,000 UGX ,
C) or more than 102,000 UGX?
Y) DON'T KNOW
Z) REFUSE TO ANSWER

PLEASE CIRCLE THE RESPONSE

Q97: If you have UGX. 100,000 in a savings account earning 1% interest per annum, and prices for goods and services rise 2% over a 1-year period, can you buy

A) more than,
B) less than,
C) or the same amount of goods in 1 year as you could today, with the money in the account?"
Y) DON'T KNOW
Z) REFUSE TO ANSWER

PLEASE CIRCLE THE RESPONSE

Q98: Is it riskier to plant

A) multiple crops or
B) one crop?
Y) DON'T KNOW
Z) REFUSE TO ANSWER

PLEASE CIRCLE THE RESPONSE

Q99:	Suppose you need to borrow. 500,000 UGX. Two people offer you a loan. Which loan represents a better deal for you? A) One loan requires you to pay back 600,000 UGX in 1 month. B) The second loan requires you to pay back in 1 month 500,000 UGX plus 15% interest. Y) DON'T KNOW Z) REFUSE TO ANSWER ***PLEASE CIRCLE THE RESPONSE***
Q100:	Suppose you owe 3,000,000 UGX to a bank. You pay a minimum payment of 30,000 UGX each month. At an Annual Percentage Rate of 12% (or 1% per month), how many years would it take to eliminate debt if you took no additional loan? A) Less than 5 years B) Between 5 and 10 years C) Between 10 and 15 years; D) Never, you will continue to be in debt; Y) DON'T KNOW Z) REFUSE TO ANSWER ***PLEASE CIRCLE THE RESPONSE***
Q101:	If you were offered a loan with 5% monthly interest rate and a loan with 20% annual interest rate, which loan would offer better value? A) 5% monthly interest rate B) 20% annual interest rate Y) DON'T KNOW Z) REFUSE TO ANSWER ***PLEASE CIRCLE THE RESPONSE***

11 NUMERACY MODULE		
	READ: These next questions ask you to do some math in your mind. Remember that this is not a test, so it doesn't matter if you are right or wrong! *NOTE: FOR THE QUESTIONS 102 to 104, "A WHILE" MEANS ABOUT 10 SECONDS, BUT YOU DO NOT NEED TO KEEP TIME – JUST ESTIMATE WHETHER THE RESPONDENT TOOK MORE OR LESS THAN 10 SECONDS TO ANSWER. AS MUCH AS POSSIBLE, DO NOT ALLOW THE RESPONDENT TO CALCULATE USING PEN AND PAPER OR A CALCULATOR.*	
Q102	What does 16 plus 12 equal?	GIVES RIGHT ANSWER (28) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ---------- 99
Q103	Imagine that five brothers are given a gift of 10,000 UGX. If the brothers have to share the money equally, how much does each one get?	GIVES RIGHT ANSWER (2,000UGX) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ----------99
Q104	Suppose you put 1,000 UGX into a free-of-charge savings account with a guaranteed interest rate of 10% per year. How much would be in the account at the end of the first year, once the interest payment is made?	GIVES RIGHT ANSWER (1,100UGX) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ----------99
Q105	If the same bicycle is on sale in two different shops at 200,000 shillings and one shop offers a discount of 30,000 shillings and the other shop offers a 10% discount: which one is the better bargain?	A discount of 30,000 UGX _____ 1 A discount of 10% _____ 2 Don't know_____ 99
Q106	If you have 4,800 UGX and friend gives you 5,800 UGX, how many UGX do you have?	_____UGX Don't know_____ 99

12 SAVING BEHAVIOR MODULE		
Q107:	Do you have any money saved? Just to clarify, savings do not have to be deposited in an account or formal institution, and they may or may not gain interest. They can be somewhere at home, hidden in a safe place, or with a friend or family member.	YES---------- 1 → ***TO Q109 (a)*** NO ----------2
Q108:	So there is no place where you are saving your money right now?	HAS SAVINGS ---------- 1 NO SAVINGS ---------- 2 → ***TO Q123***

We are interested in learning more about market vendors' savings behavior and access to financial services, so in this next section we will ask about where and how you save money. Please remember that this information is confidential – no one other than the researchers will see this information, and your name will not be connected to it.

I would like to know all of the ways and places you save money, and then I will ask you a couple more questions about each of these places or ways. Can you please tell me the places or ways that you save money right now?

HAVE THE RESPONDENT LIST OFF THE PLACES WHERE HE/SHE HAS MONEY SAVED, IN ORDER FROM MOST TO LEAST MONEY. CODE THE LOCATIONS IN EACH ROW OF THE FIRST COLUMN ("PLACE CODE") IN THE BELOW TABLE ACCORDING TO THE BELOW CODES, THEN PROCEED WITH QUESTIONS 109 – 120 FOR EACH LOCATION

01) POCKET
02) IN A "LOCAL BANK", BOX OR TIN AT HOME
03) HIDDEN AT HOME (EX. "IN MY MATTRESS")
04) IN A HOLE IN THE GARDEN
05) IN A ROTATING SAVINGS CLUB (ROSCA)
06) IN A SACCO
07) IN A TELECOM COMPANY ACCOUNT (EX. MTN MOBILE MONEY, UTL M-SENTE, ZAIN ZAP)
08) IN A GROUP ACCOUNT AT A FORMAL BANK
09) IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK
10) ANOTHER PERSON (E.G. A FRIEND, MY MOTHER) HOLDS IT
11) SAVES BY BUYING THINGS THAT CAN BE RESOLD (SUCH AS A GOAT OR A BICYCLE)
12) BY INVESTING IN MY BUSINESS
99) OTHER

	Place **CODE**	**Q109:** (b)How much money do you have saved in […]?	**Q110:** How many months ago did you first start saving in […]? F LESS THAN 1 MONTH = 0	**Q111:** How many weeks ago did you last put money in […]? IF LESS THAN 1 WEEK = 0	**Q112:** How much money did you put in […] that time?	**Q113:** How often do you put money in […]? READ RESPONSE OPTIONS 1) Often 2) Sometimes 3) Rarely 4) Never	**Q114:** How many weeks ago did you last take money out of […]? WITHIN LAST WEEK = 0 NEVER = 99 → 117	**Q115:** How much money did you take out of […]?	**Q116:** How often do you take money out of […]? READ RESPONSE OPTIONS 1) Often 2) Sometimes 3) Rarely 4) Never	**Q117:** Why do you choose […] to keep your money in? A) NO CLEAR REASON B) SAFETY/SECURITY C) EASY TO ACCESS D) DIFFICULT TO ACCESS SO WON'T SPEND E) NOT ENOUGH MONEY FOR FORMAL BANK ACCOUNT F) EARNS MONEY (E.G., INTEREST; ITEM WHICH CAN BE RESOLD FOR MORE) G) SOMEONE ELSE TOLD YOU TO H) FREE TO SAVE (NO CHARGES) Z) OTHER REASON	**Q118:** Are you saving up money in this place in order to achieve a certain plan? 1) Yes 0) No ***If 1 → NEXT ROW***	**Q119:** What is your plan or goal for what you will do with the money in […]? A) LARGE PURCHASE (INCLUDING LAND AND BUILDING A HOUSE) B) SAVE FOR EMERGENCIES C) USE TO INVEST OR PAY FOR SOMETHING THAT WILL EARN MORE MONEY D) EDUCATION COSTS FOR SELF OR OTHER E) HEALTH-RELATED COSTS FOR SELF OR OTHER Z) OTHER	**Q120:** How much money **total** do you want to save in order to achieve this plan or goal? DON`T KNOW = -99
		UGX	*MONTHS*	*WEEKS*	*UGX*	*CIRCLE ONE*	*# WEEKS*	*UGX*	*CIRCLE ONE*	*CIRCLE ALL THAT APPLY*	*CIRCLE ONE*	*CIRCLE ALL THAT APPLY*	*UGX*
1						1 2 3 4			1 2 3 4	A B C D E F G H Z	*1...0*	A B C D E Z	
2						1 2 3 4			1 2 3 4	A B C D E F G H Z	*1...0*	A B C D E Z	
3						1 2 3 4			1 2 3 4	A B C D E F G H Z	*1...0*	A B C D E Z	
4						1 2 3 4			1 2 3 4	A B C D E F G H Z	*1...0*	A B C D E Z	
5						1 2 3 4			1 2 3 4	A B C D E F G H Z	*1...0*	A B C D E Z	

Q121:	*DO NOT READ:* CHECK QUESTION 109: DID THE RESPONDENT LIST 8) "IN A GROUP ACCOUNT AT A FORMAL BANK" OR 9) "IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK"?	YES---------- 1→ ***TO Q 123*** NO ---------- 2
Q122:	*ONLY ASK IF DID NOT LIST 8) "IN A GROUP ACCOUNT AT A FORMAL BANK" OR 9) "IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK" FOR Q109:* Why do you choose not to have an account in a formal bank? *DO NOT PROBE. (Select all mentioned)*	DOES NOT HAVE ENOUGH MONEY TO OPEN ACCOUNT---------- 1 BANK IS TOO FAR AWAY---------- 2 BANKS ARE NOT SAFE OR SECURE (E.G. MAY BE ROBBED, BURN DOWN) ---------- 3 BANKS ARE NOT TRUSTWORTHY---------- 4 TOO MUCH WORK/TOO DIFFICULT TO OPEN ACCOUNT---------- 5 BANK FEES/CHARGES ARE TOO HIGH……6 DOES NOT KNOW---------- 98

13 BORROWING MODULE

Q123:	Do you think you could be able to borrow 100,000 UGX in case you want to?	YES---------- 1 NO ---------- 2 →***to 126***
Q124:	From where do you think it is most likely you could borrow this money from? *CHOOSE ONE*	FAMILY MEMBER OR FRIEND---------- 1 COMMERCIAL BANK---------- 2 SAVINGS GROUP ---------- 3 MICROFINANCE AGENCY---------- 4 MONEYLENDER---------- 5 OTHER (specify)|_____|99
Q125:	Do you think you could be able to get a loan of 1 million UGX in case you want to?	YES---------- 1 NO ---------- 2→ ***TO 128***
Q126:	From where do you think it is most likely for you to obtain such a loan? *CHOOSE ONE*	FAMILY MEMBER OR FRIEND---------- 1 COMMERCIAL BANK---------- 2 SAVINGS GROUP ---------- 3 MICROFINANCE AGENCY---------- 4 MONEYLENDER---------- 5 OTHER (please specify) _____ 99
Q127:	In the past 5 years, did it ever happen to you that you were late with any payment you owed (to a bank or moneylender or relative)?	YES---------- 1 NO ---------- 2 →***TO 130***
Q128:	How often did it happen to you that you were late with a payment in the past 5 years?	ONCE _____ 1 TWICE _____ 2 BETWEEN 3 AND 5 TIMES ___ 3 MORE THAN 5 TIMES _____ 4
Q129:	Have you ever applied for a loan and have been rejected?	NEVER_____0 ONCE _____ 1 TWICE _____2 BETWEEN 3 AND 5 TIMES __3 MORE THAN 5 TIMES _____ 4
Q130:	In general, how interested are you in financial matters?	1 …Not interested 2… interested 3… very interested

Now I would like to ask you some questions about lending and borrowing money. First let's discuss money that you borrowed, also known as loans. We define loans as money you received that you have to repay. Loans can come from a formal source such as a bank or microfinance institution, or from an informal source such as a friend, relative, money lender or local savings group.

Q131: In the past 6 months, have you borrowed money that you are or will be expected to pay back?

YES ---------- 01

NO ---------- 02 → ***TO Q143***

Please list all of the people or places you borrowed a lot of money from in the **past 6 months**?

COMPLETE TABLE BELOW FOR EACH SOURCE OF LOAN

	Q132: NAME OF SOURCE	**Q133:** CODE OF SOURCE 1)Family member or friend 2) Other Market Vendor 3) Commercial bank 4) Savings group 5) Microfinance agency 6) Moneylender 7) Workplace 9) Other	**Q134:** How many months ago did you last borrow from […]? IF LESS THAN 1 month = 0 DON'T KNOW= -99	**Q135:** How much money did you borrow from […]? DON'T KNOW= -99	**Q136:** How much money do you still need to pay back?	**Q137:** Did you or will you have to pay back more money than you borrowed? 1) Yes 2) No → to **Q 142**	**Q138:** For the extra money you will have to pay, do you know the interest rate or do you know the actual amount you have to pay? 1) Interest→ next 2)Extra amount → to **Q141**	**Q139:** What is the interest rate you paid on the money you borrowed?	**Q140:** Is this interest rate per year, month, week or day? 1) Year 2) Month 3) Week 4) Day 5) Other → to **Q142**	**Q141:** What is the extra amount of money you will have to pay on top of what you borrowed?	**Q142:** What is the main thing you used the money for? 1) Pay for emergency 2) invest in own business or farm 3) Pay primary or secondary school fees for self 4) Pay primary or secondary school fees for other person 5) Pay for university 6) Invest in other's business or farm 7) Buying non-essential things (luxury or enjoyment) 5) Basic needs (food, shelter or preventative medical costs) 9) Other
		ENTER ONE	# MONTHS	*UGX*	*UGX*	*CIRCLE ONE*	*CIRCLE ONE*	*%*	*ENTER ONE*	*UGX*	*ENTER ONE*
1						*1 2*	*1 2*				
2						*1 2*	*1 2*				
3						*1 2*	*1 2*				
4						*1 2*	*1 2*				
5						*1 2*	*1 2*				

14 LENDING MODULE

Q143	In the past **6 months**, have you given a loan to anybody, with the expectation that they pay you back?	YES---------- 1 NO ---------- 2→ ***TO Q151***

Please list all of the people you lent money to in the past **6 months**

COMPLETE TABLE BELOW FOR EACH BORROWER

	Q144:	**Q145:**	**Q146:**	**Q147:**	**Q148:**	**Q149:**	**Q150:**
	BORROWER NAME	BORROWER CODE 1) Family member or friend 2) Client/customer 9) Other *CIRCLE ONE*	How much did you lend to this person? *UGX*	How many months ago did you lend money to […]? *WITHIN LAST MONTH = 0* *DON'T KNOW = 9*	Did they or will they pay back more money than they borrowed? 1) Yes 0) No ***TO 152*** *CIRCLE ONE*	What interest rate will they have to pay back to you? *DO NOT PROBE OR HELP THE RESPONDENT CALCULATE* *DON'T KNOW = 998*	How much money are you still waiting to receive back? *DON'T KNOW = 998* *UGX*
1		1 2 9		|___|	1 0		
2		1 2 9		|___|	1 0		
3		1 2 9		|___|	1 0		
4		1 2 9		|___|	1 0		
5		1 2 9		|___|	1 0		

15 ACCESS TO LUMP SUM MODULE

Read: Thanks again for your time! Next I'd like to ask a couple questions about times you've had to spend a large amount of money

Q151:	How many months ago was the last time you had an emergency that required for you to pay your own money (such as a burial, a fire or a family member or friend falling sick)? *WITHIN THE PAST MONTH = 00* *NEVER = 99*	|___|___|months ***IF NEVER (99)→ TO Q 154***
Q152:	How much did you have to pay?	|___|___|,|___|___|___|,|___|___|___|UGX
Q153:	Where did you get the money from that you used to pay for that emergency? *MARK ALL THAT APPLY*	BORROWED---------- A USED SAVINGS---------- B SOLD SOMETHING OF YOURS---------- C GIVEN MONEY BY ANOTHER PERSON---------- D OTHER: |_____|--- Z
Q154:	How many months ago was the last time you purchased something that cost a lot of money? *WITHIN THE PAST MONTH = 00* *NEVER = 99*	|___|___|months ***IF NEVER (99)→ TO Q157***
Q155:	How much did it cost?	|___|___|,|___|___|___|,|___|___|___|UGX
Q156:	Where did you get the money from that you used to pay for that thing? *MARK ALL THAT APPLY*	BORROWED---------- A USED SAVINGS---------- B SOLD SOMETHING OF YOURS---------- C GIVEN MONEY BY ANOTHER PERSON---------- D OTHER: |_____|--- Z

	16 INVESTMENT BEHAVIOR MODULE	
Q157:	How much of your own money did you spend on investments in business in order to try to make profits in the **past 6 months**? It is okay to estimate. *PROBE. EMPHASIZE THAT YOU WANT TO KNOW TOTAL FOR ALL 6 MONTHS* *DON'T KNOW = -99*	|_____| UGX
Q158:	How did you get <u>most</u> of the money for these business-related expenses? *READ ALL RESPONSE OPTIONS* *CIRCLE ONLY ONE!!!*	Borrowed from somewhere that you will have to pay more money back to -- 1 Borrowed from somewhere that you will have to pay the same amount of money back to ---------- 2 Given money from someone else ---------- 3 Did some additional work ---------- 4 Money you already had/ savings ---------- 5 Sold something that you owned ---------- 6 Other (specify) _____ 8
	17 BIAS AND PREFERENCES MODULE	
Read: Now I would like to ask you a few questions about your behaviors and preferences. I will read out a few questions to you.		
Q159:	Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?	MOST PEOPLE CAN BE TRUSTED ---------- 1 YOU CAN'T BE TOO CAREFUL IN DEALING WITH PEOPLE ---------- 2
Q160:	Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?	THEY TRY TO TAKE ADVANTAGE OF YOU ---------- 1 THEY TRY TO BE FAIR ---------- 2
Q161:	Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?	PEOPLE TRY TO BE HELPFUL ---------- 1 PEOPLE ARE MOSTLY JUST LOOKING OUT FOR THEMSELVES ---- 2
Read: For the next few questions, please tell me how you respond: "yes, definitely", "probably", "probably not", or "no, definitely not". Remember, there are no right or wrong answers, it is just what you prefer!		
Q162:	Would you ever give money to someone else to keep for you?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Q163:	Are you willing to sacrifice if it makes people around you better?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Q164:	Are you more careful than most people of your age in the community about avoiding getting injured or sick?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Q165:	If you suddenly won 50,000 Shillings, would you share a lot of it with others?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Q166:	When you become ill, do you think it is because of fate?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Q167:	In general, do you trust people in your community?	YES, DEFINITELY ---------- 1 PROBABLY ---------- 2 PROBABLY NOT ---------- 3 DEFINITELY NOT ---------- 4
Read: For these next 4 questions, please answer either: often, sometimes, rarely or never. Again remember there is no right or wrong answer!		
Q168:	If you get money, do you tend to spend it too quickly?	OFTEN---------- 1 SOMETIMES ---------- 2 RARELY---------- 3 NEVER---------- 4→ ***TO Q170***

Q169:	Do you therefore put most of your money into a safe place in order to avoid spending it too quickly?	OFTEN---------- 1 SOMETIMES ---------- 2 RARELY---------- 3 NEVER---------- 4

Q170:	Are you generally a person who is fully prepared to take risks or do you try to avoid taking risk? (Please choose a number on a scale from 0 to 10)	0 1 2 3 4 5 6 7 8 9 10 (try to avoid risk) (Fully prepared to take risk)
Q171:	Attitudes towards risk change in different situations. When thinking about investing and borrowing, are you a person who if fully prepared to take risks or do you try to avoid taking risk? (Please choose a number on a scale from 0 to 10)	0 1 2 3 4 5 6 7 8 9 10 (try to avoid risk) (Fully prepared to take risk)

Read: Thank you. For the next two questions, you will be asked which of two options you prefer more.

Q172:	Imagine you just won 100 000 UGX in a lottery and you can invest this money in a business. There is a 50 % chance that the business is successful. If the business is successful you double the amount invested after one year. If it is not successful you will lose half the amount you invested. What fraction of the 100 000 UGX would you invest in the business?	_____UGX
Q173:	Suppose you have some money to do business, and you have the choice between two options: **Option A**: A business that can give a lot of profit every month, but there is a chance you can lose your money anytime. **Option B**: A business with less profit every month, but you can't lose your money. Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 98
Q174:	Imagine you are sick (but not dying) and you have the choice between the following two options: **Option A**: You can get some medicine today which will make you feel somewhat better, but you will continue to feel a small amount sick for another month. **Option B**: You can wait and continue to be sick a week until a better medicine is available that will make you feel entirely good again. You can only choose one medicine. Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 98

18 GiZ AND BoU-MODULE		
Q175:	How far is it from your household to the Nearest commercial bank and how long does it take you to get there?	a) Distance [_____] km b) Time to get there [_____] minutes
Q176:	How far is it from your household to the Nearest SACCO or micro-finance institution and how long does it take you to get there?	a) Distance [_____] km b) Time to get there [_____] minutes
Q177:	How far is it from your household to the Nearest savings club, money lender or ROSCA and how long does it take you to get there?	a) Distance [_____] km b) Time to get there [_____] minutes
Q178:	Do you think the following statements are true or false?	
a)	If someone buys livestock, it is only an investment if that person intends to obtain future income from it.	True _____ 1 False _____ 2
b)	When you buy on credit the goods end up being more expensive than they would be if bought on cash.	True _____ 1 False _____ 2

Q179:	How safe do you consider the following places for saving money?	
a)	Regulated Financial Institutions (Banks, MDIs, Credit Institutions)	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99
b)	SACCO	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99
c)	VSLA	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99
d)	At home	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99
Q180:	Under which of the following circumstances would you consider a loan as bad? *PROBE ALL ANSWERS!* *CIRCLE ALL THAT THE RESPONDENT THINKS APPLIES*	a) No clear plan of how the money will be used b) No plan on how the money will be paid back c) The money is used for the intended purpose d) The money is used for paying back another debt e) The money is used for luxuries f) The money is used for every-day expenses g) The money is used for productive investment h) Borrowing because others are borrowing
Q181:	If you were offered a loan but you were not sure what you would use it for, would you still take it?	Yes _____ 1 No _____ 0 Don't know _ 99
Q182:	In case you are dissatisfied with a financial service provider and you complain, do you think that the financial service provider is more powerful than you, and that the complaint will therefore not lead to anything?	Yes _____ 1 No _____ 0 Don't know _ 99
Q183:	Please tell me how strongly you agree or disagree with the following statements, which other people have made about taking out financia products	
a)	I am confident enough to approach a bank and ask questions to learn more about their products.	Disagree strongly ___ 1 Tend to agree _____ 2 Agree strongly _____ 3 Don't know _____ 99
b)	I am confident that among a range of loans offered by different banks, I can choose the loan that best suits my specific needs.	Disagree strongly ___ 1 Tend to agree _____ 2 Agree strongly _____ 3 Don't know _____ 99
Q184:	Which of the following statements best describes how you last chose a financial product (loan, account, policy…)? *CHOOSE ONE!!!*	I considered several products from different companies before making my decision _____ 1 I considered the various products from one company _____ 2 I didn't consider any other products at all _____ 3 I looked around but there were no other products to consider ___ 4

Have you heard of any of the following financial products?
If so, do you currently hold or use any of these types of products (personally or jointly)?

Q185:	Bank loan	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q186:	Microfinance loan	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q187:	Mortage	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q188:	Savings account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q189:	Current account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q190:	Fixed deposit account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q191:	Overdraft	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0

Q192:	Pension fund	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q193:	Insurance	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q194:	Cheque book	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q195:	Credit card	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q196:	ATM card / debit card	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q197:	Mobile money account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q198:	Cell phone banking (with a bank account)	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q199:	Money transfer services (Western Union, Money Gram)	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0

19 PARTICIPATION IN OTHER PROGRAMS		
Read: Now I want us to talk about programs that you or your community received in the past from CBOs, NGOS or the government..		
Q200: In the past 12 months, aside from our visit, did you receive help from any other program?	1. Yes 0. No → **Q204**	
Q201: What was the program?	1. Government 2. NGO 3. CBO 96. Other	___
Q202: Fill in the name of the program:		
Q203: What did you receive?	a. Cash b. Training c. Counseling	d. Medical aid e. Other
Q204: In the next 12 months, how likely do you think it is that people in this community will receive any help from a program?	1. Very likely 2. Somewhat likely 3. Not very likely 4. Not at all likely	
Q205: Have you heard about the national strategy for financial literacy in Uganda?	1. yes 0. no	
Q206: Have you ever seen TV-ads or ads in newspapers by the Bank of Uganda concerning issues on money management?	1. yes 0. no→ **Q208**	
Q207: From your memory, how often have you seen these TV or newspaper-ads?		__times
Q208: Have you ever seen TV-ads or ads in newspapers by other private institutions concerning issues on personal money management?	1. yes 0. no →**Q210**	
Q209: From your memory, how often have you seen these TV or newspaper-ads?		__times

20 RISK- AND TIME-PREFERENCES MODULE		
***Read*:** We are almost done with the interview, and we appreciate your patience. In these last few questions, we will play a fun exercise that will involve potential payouts with real money. One of these questions will be chosen to be actually be paid at the end of our session, so be careful about which option you choose for each question, since that one might be chosen for you to play to receive money, and if so you will not be able to change your answer! In this activity, we will ask you two types of questions in which you choose between different options. In the first questions, you will be asked to choose between games of zala in which you can win different amounts of money. Zala (or labyeka) is a child's activity some of you may remember. In the activity, I have a stone in one hand, and you do not know which hand. You must then guess the hand with the stone. If you guess correctly, you win. If you do not guess correctly, you lose. You will receive money based on your answers to 1 randomly selected question in the following exercise. Take care in the choices you make for all questions, because once you have answered all of the questions, we will reveal which question has been randomly selected to be performed with real money. We will then use the responses you have selected for those questions to determine the actual payment. You will not be able to change your responses once we reveal which questions have been selected. This is not our personal money. Rather, it is money given to us by the research organization, to do this activity in order to better understand your preferences. In other questions, the options involve decisions about receiving money now or receiving money later. These questions will not be selected for payout. Do you have any questions before we proceed? *ANSWER ANY QUESTIONS*.		
Q210:	**(1)** Imagine you have a choice between the following two options: Option A: You can receive **900** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q211:	**(2)** Now imagine you have a choice between the following two options: Option A: You can receive **600** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q212:	**(3)** Now imagine you have a choice between the following two options: Option A: You can receive **1,200** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q213:	**(4)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **3,000** Shillings. If you lose, you get **2000** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **1000** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q214:	**(5)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **2,000** Shillings. If you lose, you get **1500** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **1000** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q215:	**(6)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **3,000** Shillings. If you lose, you get **2000** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **0** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q216:	Now imagine you have a choice between the following options: Option A: you get **2,000** Shillings immediately Option B: you get **6,000** Shillings in two weeks Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8

Q217:	Now imagine you have a choice between the following options: Option A: you get **2,000** Shillings immediately Option B: you get **8,000** Shillings in two weeks Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q218:	Now imagine you have a choice between the following options: Option A: you get **2,000** Shillings immediately Option B: you get **4,000** Shillings in two weeks Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q219:	Now imagine you have a choice between the following options: Option A: you get **2,000** Shillings in two weeks Option B: you get **6,000** Shillings in four weeks Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8

Read: Thank you very much for your participation. Now we are going to play one of the games above for real money. Which of the games we play is determined randomly by throwing a dice.

ENUMERATOR: THROW A DICE (ONLY ONCE!). THE NUMBER ON THE DICE INDICATES WHICH OF THE 6 GAMES ABOVE (MARKED WITH (1) TO (6) AT THE BEGINNING OF THE QUESTION WILL BE PLAYED. WRITE DOWN WHICH GAME WAS CHOSEN BELOW. THEN CHECK WHICH OPTION OR GAME THE PARTICIPANT CHOSE FOR THIS QUESTION. IF THE PARTICIPANT CHOSE THE SURE AMOUNT, TELL HIM THAT HE CHOSE THE SURE AMOUNT AND WRITE DOWN THE AMOUNT IN THE FIELD BELOW.

IF THE PARTICIPANT CHOSE THE GAME, TELL HIM THAT YOU ARE GOING TO PLAY A ROUND OF ZALA/ LABYEKA NOW. THEN PLAY ONE ROUND (AND ONLY ONE. DO NOT REPEAT THE GAME!) IF THE PARTICIPANT WINS, CHECK IN THE DESCRIPTION OF THE GAME ABOVE HOW MUCH HE WINS, TELL HIM HOW MUCH IT IS AND WRITE DOWN THE AMOUNT HE WINS BELOW. IF HE LOSES, CHECK HOW MUCH MONEY THE PARTICIPANT RECEIVES IN THE DESCRIPTION OF THE GAME ABOVE, TELL THE PARTICIPANT HOW MUCH HE RECEIVES AND WRITE DOWN THE AMOUNT BELOW. IF THE RESPONDENT CHOSE "NO PREFERENCE", ROLE THE DICE AGAIN ONCE. IF THE DICE SHOWS NUMBER 1,2 OR 3, PLAY OPTION A OF THE GAME AS DESCRIBED ABOVE. IF THE DICE SHOWS NUMBER 4, 5 OR 6, PLAY OPTION B OF THE GAME AS JUST DESCRIBED ABOVE.

Q220:	Number on the dice	_____
Q221:	Payment amount:	__ __ __ __ USH

20. RESPONDENT TRACKING INFORMATION MODULE

Remember we would like to conduct a similar survey with you about three/four months from now. At that time, you will again be free to decide you do not want to participate in the survey. We'd like to ask you some information about yourself and people you know so that we will be able to find when we return.

Q222:	Q What is your surname? *WRITE IN CLEAR BLOCK LETTERS, CHECK*	| |
Q223:	What is your first name? *CLEAR BLOCK LETTERS, CHECK SPELLING*	| |
Q224:	What is your other name? *IF NO OTHER NAME, LEAVE BLANK*	| |
Q225:	What is your address?	|_____|
Q226:	What is the name of the village where you usually	|_____|
Q227:	What is this village's subcounty?	|_____|
Q228:	What is this village's parish?	|_____|
Q229:	What is/are your phone numbers? *PHONE 1*	0|__|__|__| - |__|__|__|__|__|__|__|
Q230:	*PHONE 2*	0|__|__|__| - |__|__|__|__|__|__|__|
Q231:	*PHONE 3*	0|__|__|__| - |__|__|__|__|__|__|__|

Q232:	Is there some way to describe where you live? (For example, near the old Catholic church or under the large mango tree on the hill)	

What are the names and contact information of two people who will likely know where you are that we can contact for information on next visit?

Q233:	*CONTACT 1 NAME*	|_____|	CONTACT 1
Q234:	*CONTACT 1 RELATIONSHIP TO RESPONDENT*	|_____|	
Q235:	*CONTACT 1 PHONE NUMBER*	0|___|___|___| - |___|___|___|___|___|___|	
Q236:	*CONTACT 1 ADDRESS*	|_____|	
Q237:	*CONTACT 2 NAME*	|_____|	CONTACT 2
Q238:	*CONTACT 2 RELATIONSHIP TO RESPONDENT*	|_____|	
Q239:	*CONTACT 2 PHONE*	0|___|___|___| - |___|___|___|___|___|___|	
Q240:	*CONTACT 2 ADDRESS*	|_____|	

Read the following conclusion:

We are now finished with the interview. Thank you very much for your time.

The results of this survey will help interested parties understand how to better design decentralized development programs.

Do you have any questions for me before I leave?	***Briefly answer any questions that the respondent asks, then complete the rest of the survey once the respondent has left.***	
Q241: Time interview ends	HH:MM: |___|___|:|___|___| AM/PM	
Q242: Result codes (*CIRCLE ONE):* 01. Completed 04. Partly completed 02. Postponed 05. Incapacitated 03. Refused 06. Other_____		
Q243: ***Do not read:*** ARE YOU SUSPICIOUS THAT THIS PERSON WAS INTOXICATED DURING THIS INTERVIEW?	1. No, completely sober 2. A little bit intoxicated 3. Somewhat intoxicated 4. Totally intoxicated	
Q244: ***Do not read:*** *HOW DID THIS PERSON'S THOUGHT PROCESS APPEAR TO YOU DURING THE SURVEY?*	1. Logical and sensible 2. A little unclear or confused 3. Several times unclear or insensible 4. Totally unclear or disoriented	
Q245: ***Do not read:*** *(a) Do you feel that this person needs to be referred for emergency assistance?*	1. Yes → ***Explain in part b and c*** 2. No	(b) For what reason? (c) How urgent? 1. Emergency (right away) 2. Moderate priority 3. Low priority
Notes		

FL-Impact Evaluation: Follow-Up Questionnaire (English)

PLEASE CHECK THE FOLLOWING INFORMATION CAREFULLY. IT SHOULD HELP YOU TO MAKE SURE YOU FOUND THE RIGHT PERSON.

A1	Market ID code	|__| __| __|
A2	Respondent's name	
A3	Respondent's phone number	
A4	Unique respondent ID code (****copy from the respondent tracking sheet****)	|__| __| __| __| __| __| __| (7-digit code)

ONLY TO BE FILLED BY THE SUPERVISOR:

B1 SUPERVISOR ID: |__|__| |__|__|__|__|__|__|

B2 DATE: DD/MM//YYYY): _ _/_ _/_ _ _ _

RESPONDENT IDENTIFICATION DETAILS ARE CORRECT (CHECK WITH RESPONDENT TRACKING SHEET):

B3 |__| yes |__| no → *CHECK QUESTIONNAIRE AND ADD THE MISSING INFORMATION.*

SIGNATURE:

_____

ONLY TO BE FILLED BY THE DATA ENTRANT:

C1 DATA ENTRANT ID: |__|__| |__|__|__|__|__|__|

C2 NUMBER OF QUESTIONNAIRE ENTERED: __ __ __

C3 DATE OF DATA ENTRY (DD/MM//YYYY): _ _/_ _/_ _ _ _

C4 TIME DATA ENTRY STARTED: _ _ : _ _

C5 TIME DATA ENTRY ENDED: _ _ : _ _

SIGNATURE:

_____

FL-Impact Evaluation: Follow-Up Questionnaire (English)

Q1: (a) Date of Interview: DD/MM/YYYY: |___|___|/|___|___|/|___|___|___|___|		
(b) Time of Interview: HH:MM: |___|___|:|___|___|		
Q2: Market ID Code: _ _ _		
Q3: Respondent ID Code: _ _ (Rolling number)		
Q4: (a) Enumerator's details	Name:	ID Code: _____
(b) Supervisor's details	Name:	ID Code: _____
Q5: Location of interview:		
(a) District: _ _	(b) County: _ _	
(c) Sub-county: _ _ _ _ _ _ _	(d) Town: _ _ _ _ _ _ _ _ _ _ _ _ _ _ _ _	
(e) Parish: _ _ _ _ _ _ _	(f) Village/Neighborhood(88=N/A): _ _ _ _ _ _ _ _ _ _ _ _ _ _ _	
(g) Urban=1, Rural=2: _ _		
(h) Name of Market location: _ _ _ _ _ _ _ _ _ _ _ _		
Q6: ***Do not read:*** DOES THIS PERSON SEEM EMOTIONALLY AND MENTALLY CAPABLE OF COMPLETING THIS SURVEY?	1. Yes 2. No, intoxicated 3. No, mentally impaired 4. No, other: _____ ***If no, stop interview and refer the case to the survey supervisor***	
Q7: ***Do not read:*** IS THE INTERVIEW BEING CONDUCTED WITH THE INTERVIEWEE ALONE (EXCEPTING SURVEY STAFF)?	1. Yes 2. No → ***Politely ask to be allowed to interview the respondent alone. Stress that the interview is private and confidential.***	
Q8: ***Do not read:*** ARE YOU SUSPICIOUS THAT THE PERSON THAT YOU ARE INTERVIEWING IS NOT THE ONE WE SOUGHT FOR AN INTERVIEW?	0. No, not at all 1. A little suspicious → ***Explain*** 2. Very suspicious → ***Explain***	(b) WHY ARE YOU AT ALL SUSPICIOUS? _____

1 DEMOGRAPHICS				
Q9:	*GENDER*	MALE---------- 1 FEMALE---------- 2		
Q10:	What is your tribe? *ENTER THE CORRECT 2-DIGIT CODE IN THE SPACES AT FAR RIGHT. IF OTHER, WRITE TRIBE IN "OTHER" SPACE AND FILL 99 CODE*	01) BAAMBA 02) BABWISI 03) BAGANDA 04) BAKHONZO 05) BAKIGA 06) BASONGORA 07) BATORO 08) BATUKU	09) BANYAKOLE 10) BANYARWANDA 11) BANYOLE 12) BANYORO 13) CONGOLESE 14) IK (TEUSO) 15) ITESO 16) RWANDESE 99) OTHER: _____	|___|___|
Q11:	How old are you?	|___|___| years		
Q12:	What is your religion? *PLEASE CIRCLE ONE*	01) Roman Catholic 02) Church of Uganda (Anglican) 03) Pentecostal 04) Muslim 99) Other		
Q13:	What is the highest level you have completed in school? *ENTER THE CORRECT 2-DIGIT CODE IN THE SPACES AT FAR RIGHT.*	01) none 02) P1 08) P7 14) S6 03) P2 09) S1 15) SOME TERTIARY INSTITUTE 04) P3 10) S2 16) COMPLETED TERTIARY INSTITUTE 05) P4 11) S3 17) SOME UNIVERSITY 06) P5 12) S4 18) COMPLETED UNIVERSITY 07) P6 13) S5 19) MASTER'S OR PHD		|___|___|
Q14:	Can you read or write in any language? *PLEASE CIRCLE ONE*	1) Neither able to read or write 2) Able to read only 3) Able to read and write		
Q15:	Have you received Financial Literacy training during our last visit?	YES----------1 NO ----------2		

	2 DEPENDENCY PROFILE	
Q16:	How many children do you support, including those children who are not biologically yours? By "children" I mean those who are less than 18 years old.	|___|___|children ***IF NONE (00)***
Q17:	How many other people who are not children do you support? These are people who are at least 18 years old. Remember by "support" I mean you regularly give them financial assistance that they do not have to work for.	|___|___|people ***IF NONE (00)***

	3 HOUSEHOLD PROFILE MODULE **Read**: Now I would like to know how many people are in your household. The household is defined as all of the people who normally live and eat their meals together in your home (this does not include visitors).		
Q18:	How many people are in your household (including yourself)?	|___|___|people	
Q19:	Who do you usually stay with in your Household (excluding visitors)? *READ OUT ALL RELATIONS. ENTER THE NUMBER(QUANTITY) OF EACH RELATION WHO THE RESPONDENT STAYS WITH. PROBE TO BE SURE YOU ARE ENTERING THE CORRECT NUMBER FOR EACH RELATION.* *ENTER "00" FOR ALL THOSE THAT DO NOT APPLY*	(a) OWN CHILDREN | _ _ | (b) BIOLOGICAL MOTHER | _ _ | (c) BIOLOGICAL FATHER | _ _ | (d) ADOPTIVE (LEGALLY) MOTHER | _ _ | (e) ADOPTIVE (LEGALLY) FATHER | _ _ | (f) STEP-MOTHER | _ _ | (g) STEP-FATHER | _ _ | (h) AUNT | _ _ | (i) UNCLE | _ _ | (j) OLDER SISTER | _ _ | (k) OLDER BROTHER | _ _ |	(l) YOUNGER SISTER | _ _ | (m) YOUNGER BROTHER | _ _ | (n) GRANDMOTHER | _ _ | (o) GRANDFATHER | _ _ | (p) SPOUSE | _ _ | (q) MOTHER IN LAW | _ _ | (r) FATHER IN LAW | _ _ | (s) OTHER FAMILY MEMBER | _ _ | (t) OTHER NON-FAMILY MEMBER | _ _ |
Q20:	So this means that including yourself, **XX** (***add up numbers and add the respondent himself***) people are staying at your place?	Yes, there are __ people staying in my household ***If not, make corrections above and ask again***	
Q21:	How many people regularly contribute some money to the household (including yourself)?	|___|___|people	

Q22:	Who contributes the most money to the household, in order to regularly meet the household's basic needs? *COMPLETE 22A IN COLUMN **A** ("CONTRIBUTER") AT THE RIGHT FOR THIS PERSON*	01) RESPONDENT HIM/HERSELF 02) BIOLOGICAL MOTHER 03) BIOLOGICAL FATHER 04) ADOPTIVE MOTHER 05) ADOPTIVE FATHER 06) STEP-MOTHER 07) STEP-FATHER 08) AUNT 09) UNCLE 10) OLDER SISTER 11) OLDER BROTHER 12) YOUNGER SISTER 13) YOUNGER BROTHER 14) GRANDMOTHER 15) GRANDFATHER 16) SPOUSE 99) OTHER	**A** **Contributer** 22A |___|___|	
Q23:	Who is the head of the household? *COMPLETE 23B IN COLUMN **B** ("HEAD") AT THE RIGHT FOR THIS PERSON. IF THIS IS THE SAME PERSON AS 22A, ENTER THE SAME INFORMATION.*	01) RESPONDENT HIM/HERSELF 02) BIOLOGICAL MOTHER 03) BIOLOGICAL FATHER 04) ADOPTIVE MOTHER 05) ADOPTIVE FATHER 06) STEP-MOTHER 07) STEP-FATHER 08) AUNT 09) UNCLE 10) OLDER SISTER 11) OLDER BROTHER 12) YOUNGER SISTER 13) YOUNGER BROTHER 14) GRANDMOTHER 15) GRANDFATHER 16) SPOUSE 99) OTHER		**B** **Head** 23B |___|___|
Q24:	What is the highest level of education [*READ RESPONSE TO 22A, THEN REPEAT FOR 23B*] has reached? *ENTER THE CODE AT RIGHT FOR EACH PERSON. CHECK WITH ABOVE TO ENSURE YOU FILL THE CORRECT INFORMATION FOR THE CORRECT PRESON*	01) NONE 02) SOME PRIMARY 03) COMPLETED PRIMARY 04) SOME SECONDARY 05) COMPLETED SECONDARY (S6) 06) SOME TERTIARY INSTITUTE 07) COMPLETED TERTIARY INSTITUTE 08) SOME UNIVERSITY 09) COMPLETED UNIVERSITY 10) MASTER'S OR PHD 98) DON'T KNOW	**24A** |___|___|	**24B** |___|___|
Q25:	What is the **MAIN** way [*READ RESPONSE TO 22A (Contributer), THEN REPEAT FOR 23B (Head)*] gets money? *ENTER THE CORRECT 2-DIGIT CODE AT RIGHT FOR EACH PERSON. CHECK WITH ABOVE TO ENSURE YOU FILL THE CORRECT INFORMATION FOR THE CORRECT PRESON*	01) VENDING OF FOOD OR ITEMS AT THE MARKET 02) SUBSISTENCE FARMER/AGRICULTURE 03) COMMERCIAL FARMER/AGRICULTURE 04) MAKING BRICKS FOR SALE 05) MAKING CHARCOAL FOR SALE 06) COLLECTING FIREWOOD OR GRASS FOR SALE 07) DIGGING IN SOMEONE ELSE'S GARDEN 08) TAKING CARE OF SOMEONE ELSE'S 100,000UGX ANIMALS 09) TAKING CARE OF OWN ANIMALS 10) BREWING ALCOHOL/BEER 11) MONEY-LENDING 12) BODA-BODA/TAXI DRIVING 13) FISHING 14) QUARRYING 15) SMALL-SCALE VOCATION (EX. METAL-WORK, CARPENTRY, SHOE-REPAIR, SEWING) BUSINESS 16) SALOON (CUTTING, PLAITING HAIR) 17) HEALTH OR NGO WORKER 18) SOLDIER/POLICEMAN 19) TEACHER OR OTHER PUBLIC SERVANT 20) POLITICAL POSITION 21) WORK IN ANOTHER PERSON'S HOME (EX. ASCARI, MAID) 22) RENTING LAND 23) SMALL-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH LESS THAN 100,000 UGX 24) LARGER-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH MORE THAN 25) NON-SALARIED (WAGE-EARNING FOR HOURS WORKED) EMPLOYEE IN CHURCH 26) SALARIED EMPLOYEE IN CHURCH 27) OTHER WAGE EMPLOYMENT (CASUAL LABOR – MONEY EARNED FOR HOURS WORKED OR JOB COMPLETED) 28) SALARIED EMPLOYEE IN A COMPANY OR FIRM 29) OTHER SMALL BUSINESS OWNER – ASSETS VALUED AS LESS THAN 100,000UGX 30) OTHER LARGER BUSINESS OWNER – BUSINESS ASSETS VALUED AS MORE THAN 100,000 UGX 31) TRANSFERS FROM OTHER PEOPLE 98) DON'T KNOW 99) OTHER: [SPECIFY IN RESPONSE SPACE] _____	**25A** |___|___|	**25B** |___|___|

Q26: How many plots of land does your household own? *PLEASE ONLY LIST PLOTS THAT YOU YOURSELF OR YOUR HOUSEHOLD MEMBERS OWN. REMEMBER, THE HOUSEHOLD MEMBERS ARE THE PEOPLE THAT NORMALLY LIVE AND EAT THEIR MEALS TOGETHER IN YOUR HOME.*	[____] number of plots

Q27: Read: Now I want to ask you about the items owned by you and your household. I want to remind you that the purpose of this survey is not to provide assistance, so please respond fully and completely, as your answers will not affect any kind of benefits.

How many of each of the following items do you and your household own? ***Read each asset off list and write number***

|__|__| a) Donkeys	|__|__| p) Motor vehicle (car or truck)	|__|__| ae) Car batteries
|__|__| b) Oxen	|__|__| q) Mobile phones	|__|__| af) Generator
|__|__| c) Cattle (excluding oxen)	|__|__| r) Beds	|__|__| ag) Sewing machines
|__|__| d) Goats	|__|__| s) Sofas	|__|__| ah) Boat or canoe
|__|__| e) Sheep	|__|__| t) Chairs	|__|__| ai) Radios
|__|__| f) Pigs	|__|__| u) Water-heaters	|__|__| aj) Cassette or CD players
|__|__| g) Chickens and Turkey	|__|__| v) Tables	|__|__| ak) Televisions
|__|__| h) Ducks and Guinea fowls	|__|__| w) Stools	|__|__| al) Video cassette or DVD player
|__|__| i) Doves and pigeons	|__|__| x) Mattresses	|__|__| am) Laptop or desktop computer
|__|__| j) Hoes	|__|__| y) Blankets	|__|__| an) Wheelbarrow
|__|__| k) Pangas	|__|__| z) Kettle	|__|__| ao) Speakers
|__|__| l) Granary	|__|__| aa) Iron	|__|__| ap) Helmets
|__|__| m) Bicycles	|__|__| ab) Jerry cans	|__|__| aq) Mirrors
|__|__| n) Motorcycles	|__|__| ac) Pots and pans	|__|__| ar) Watches
|__|__| o) Stoves	|__|__| ad) Fans	|__|__| as) Other ***(Specify in Q28 –for items that they feel are very important)***

Q28: If "Other" describe:

4 ECONOMIC ACTIVITIES MODULE

Now I want to ask you about what work **you** did to earn money during the last 30 days. Please take a moment to think about what work you did to earn money in that time. Please tell me the activities that you got money from in these months.

01) VENDING OF FOOD OR ITEMS AT THE MARKET
02) SUBSISTENCE FARMER/AGRICULTURE
03) COMMERCIAL FARMER/AGRICULTURE
04) MAKING BRICKS FOR SALE
05) MAKING CHARCOAL FOR SALE
06) COLLECTING FIREWOOD OR GRASS FOR SALE
07) DIGGING IN SOMEONE ELSE'S GARDEN
08) TAKING CARE OF SOMEONE ELSE'S
09) TAKING CARE OF OWN ANIMALS
10) BREWING ALCOHOL/BEER
11) MONEY-LENDING
12) BODA-BODA/TAXI DRIVING
13) FISHING
14) QUARRYING
15) SMALL-SCALE VOCATION (EX. METAL-WORK, CARPENTRY, SHOE-REPAIR, SEWING)
16) SALOON (CUTTING, PLAITING HAIR)
17) HEALTH OR NGO WORKER
18) SOLDIER/POLICEMAN
19) TEACHER OR OTHER PUBLIC SERVANT
20) POLITICAL POSITION
21) WORK IN ANOTHER PERSON'S HOME (EX. ASCARI, MAID)
22) RENTING LAND
23) SMALL-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH LESS THAN 100,000 UGX
24) LARGER-SCALE RETAILER (SOMEONE WHO BUYS THINGS TO RESELL) – BUSINESS ASSETS WORTH MORE THAN 100,000UGX ANIMALS
25) NON-SALARIED (WAGE-EARNING FOR HOURS WORKED) EMPLOYEE IN CHURCH
26) SALARIED EMPLOYEE IN CHURCH
27) OTHER WAGE EMPLOYMENT (CASUAL LABOR – MONEY EARNED FOR HOURS WORKED OR JOB COMPLETED)
28) SALARIED EMPLOYEE IN A COMPANY OR FIRM
29) OTHER SMALL BUSINESS OWNER – BUSINESS ASSETS VALUED AS LESS THAN 100,000 UGX
30) OTHER LARGER BUSINESS OWNER – BUSINESS ASSETS VALUED AS MORE THAN 100,000 UGX
31) TRANSFERS FROM OTHER PEOPLE
98) DON'T KNOW
99) OTHER: [SPECIFY IN RESPONSE SPACE] _____

	Q29: *ACTIVITY CODE FROM ABOVE*	**Q30:** How much money did you get for [*ACTIVITY*] in the last 30 days? It's okay to estimate. *DON'T KNOW = 98* *RESPONSE IN UGX*	**Q31:** Regarding the money your received for [ACTIVITY], was this a normal month, a good month (income above the average) or a bad month (income below the average)?	**Q32:** In which months did you do [*ACTIVITY*]? A) January B) February C) March D) April E) May *CIRCLE ALL THAT APPLY*	**Q33:** During the last 30 days, during how many days did you spend some time doing [*ACTIVITY*]? It is okay to estimate. *HELP RESPONDENT ESTIMATE*
1	|__|__|	|__|,|__|__|__|,|__|__|__|	A) normal month B) good month C) bad month	A B C D E	|__|__| days
2	|__|__|	|__|,|__|__|__|,|__|__|__|	A) normal month B) good month C) bad month	A B C D E	|__|__| days
3	|__|__|	|__|,|__|__|__|,|__|__|__|	A) normal month B) good month C) bad month	A B C D E	|__|__| days
4	|__|__|	|__|,|__|__|__|,|__|__|__|	A) normal month B) good month C) bad month	A B C D E	|__|__| days
5	|__|__|	|__|,|__|__|__|,|__|__|__|	A) normal month B) good month C) bad month	A B C D E	|__|__| days

Q34: Do you keep a log or record of expenses and revenues for any of your businesses?	1. Yes 0. No
Q35: Have you formally registered any of your businesses with regulatory authorities?	1. Yes 0. No
Q36: Do you pay any business taxes?	1. Yes 0. No

Q37: What is your main income-generating activity? ***[Record Activity using code from Q29]***	|__ __ | (2-digit activity code from **Q29**)
Q38: How satisfied are you with your main income generating activity ***[say activity from Q37]?***	1. Very satisfied 2. Somewhat satisfied 3. Somewhat unsatisfied 4. Very unsatisfied
Q39: In the last month, did you do any subsistence farming?	1. Yes, every day 2. Yes, some days per week 3. Yes, once per week 4. Yes, once per month 5. No

Q40:	How much money did you get in total in the last month that you did not work for (for example as inheritance, gift, remittance, …) ? *PROBE - ENCOURAGE RESPONDENT TO ESTIMATE DON'T KNOW = -9,999,998*	|___|,|___|___|___|,|___|___|___|UGX
Read: Now I would like to ask you some questions related to your vending activities in the market.		
Q41: For how many years have you been vending food, vegetables or other items on markets?		__ __ years
Q42: For how many years have you been vending in this particular market?		__ __ years
Q43: Are you vending regularly on this market?		1) Yes, whenever the market takes place. 2) Whenever I have something to sell. 3) I only rarely attend this market as a vendor.
Q44: Are you also vending items in another market?		1) Yes, more than once a week. 2) Yes, once a week. 3) Yes, once a month or more (but less often than once a week) 4) Yes, but less than once a month 5) No → ***go to Q46***
Q45: Please tell me the location of the markets on which you are vending most often. *WRITE DOWN MARKET NAME AND LOCATION*		1) Location of market attended most often as a vendor _____ 2) Location of market attended second most often as a vendor _____ 3) Location of market attended third most often as a vendor _____
Q46: What type of items are you selling on the market?		1) Fresh food items 2) Cooked food items 3) Non-food items 4) Livestock 99) Other (Specify): _____
Q47: From your house, how long does it take you to get to this market?		1) Less than 30 minutes on foot. 2) More than 30 minutes on foot but less than 30 minutes by boda/motor-vehicle. 3) More than 30 minutes by boda/motor-vehicle. 98) Don`t know

5 CONSUMPTION AND EXPENDITURES

Now I am going to ask you some questions about the consumption of your household. First, for each one of the following food items, we want to know about consumption of the item that took place within your household. We are also not asking about expenditure, but rather we want you to value the actual amount that you and your household consumed in the past 7 days. For example, imagine that your household purchased a bag of rice 7 days ago for 4000 USH. If your household has not yet consumed any of it, then the consumption value of this rice would be 0. If your household has consumed half of the bag, then the consumption value would be 2000 USH. Your consumption calculation should also include items that were produced at home, or given to your household as gifts.

	Q48:	**Q49:**	**Q50:**	**Q51:**
	In the **past 7 days**, did members of the household eat/drink [item] within the household?	Did you buy this item or did you consume It from your own production (i.e garden, own field, own farm)?	How many days did the household consume [item] in the **past 7 days**?	If you had to buy the exact amount of [item] consumed by your household in the **past 7 days**, how much would it cost?
	1. Yes 0. No	1. bought the item 2. produced the item myself	__ __ days	_ _ _ _ _ _USH
a) Cassava				|__|,|__|__|__|,|__|__|__|
b) Potatoes (sweet, irish, yams)				|__|,|__|__|__|,|__|__|__|
c) Rice				|__|,|__|__|__|,|__|__|__|
d) Meat and chicken (beef, pork, goat, bush meat)				|__|,|__|__|__|,|__|__|__|
e) Fish				|__|,|__|__|__|,|__|__|__|
f) Eggs				|__|,|__|__|__|,|__|__|__|
g) Posho				|__|,|__|__|__|,|__|__|__|
h) Matooke				|__|,|__|__|__|,|__|__|__|
i) Millet bread				|__|,|__|__|__|,|__|__|__|
j) Palm Oil				|__|,|__|__|__|,|__|__|__|
k) Sorghum flour				|__|,|__|__|__|,|__|__|__|
l) Sliced bread and buns				|__|,|__|__|__|,|__|__|__|
m) Beans and peas				|__|,|__|__|__|,|__|__|__|
n) Porridge/Bushera				|__|,|__|__|__|,|__|__|__|
o) Milk				|__|,|__|__|__|,|__|__|__|
p) Fruits (bananas, apples, oranges, mangos, jackfruit, pineapple, etc.)				|__|,|__|__|__|,|__|__|__|
q) Vegetables (tomatoes, onions, cabbage, dodo, avocado, popo etc.)				|__|,|__|__|__|,|__|__|__|
r) Infant formula foods				|__|,|__|__|__|,|__|__|__|
s) Oil/butter				|__|,|__|__|__|,|__|__|__|
t) Sodas				|__|,|__|__|__|,|__|__|__|
u) Ground nuts (in shell, pounded, pasted)				|__|,|__|__|__|,|__|__|__|
v) Salt				|__|,|__|__|__|,|__|__|__|
w) Sugar				|__|,|__|__|__|,|__|__|__|
x) Tea				|__|,|__|__|__|,|__|__|__|
y) Coffee				|__|,|__|__|__|,|__|__|__|
z) Alcohol				|__|,|__|__|__|,|__|__|__|

Read: Now, I would like to know about outside consumption of food and drink. That is, I would like you to estimate, to the best of your knowledge, the value of the household's consumption of the following goods that took place outside of the household. Please include gifts as well as purchases.

	Q52:	**Q53:**	**Q54:**
	Within the **past 7 days**, did the members of this household take any [item] outside the household?	How many days did the household or any member spend on [item] in the **past 7 days**?	What was the total value of [item] eaten/drunk outside of the household in the **past 7 days**?
	1. Yes 0. No	_ _ days	_ _ _ _ _ USH
a) Full meals (breakfast, lunch, or dinner)			|__|,|__|__|__|,|__|__|__|
b) Snacks (chapati, chips, cassava, samosas…)			|__|,|__|__|__|,|__|__|__|
c) Muchomo (chicken, goat, pork, beef)			|__|,|__|__|__|,|__|__|__|
d) Sodas and juices			|__|,|__|__|__|,|__|__|__|
e) Arege (local liquor)			|__|,|__|__|__|,|__|__|__|
f) Kwete (local beer)			|__|,|__|__|__|,|__|__|__|
g) Wine, commercial beer and liquor			|__|,|__|__|__|,|__|__|__|

Q55: Now I will read you a list of items. For each item or expense, please tell me how much your household spent on the item in the **past 4 weeks**.

Item	*USH*
a) Charcoal/coal	|__|,|__|__|__|,|__|__|__|
b) Firewood	|__|,|__|__|__|,|__|__|__|
c) Kerosene/paraffin	|__|,|__|__|__|,|__|__|__|
d) Other fuel	|__|,|__|__|__|,|__|__|__|
e) Matches, lighters, candles, lamp/stove wicks	|__|,|__|__|__|,|__|__|__|
f) Laundry soap, toilet soap	|__|,|__|__|__|,|__|__|__|
g) Cigarettes or tobacco	|__|,|__|__|__|,|__|__|__|
h) Airtime, internet, and phone-charging	|__|,|__|__|__|,|__|__|__|
i) Public transportation – bus, taxi fares, petrol, boda boda	|__|,|__|__|__|,|__|__|__|
j) Newspapers or magazines	|__|,|__|__|__|,|__|__|__|
k) Batteries	|__|,|__|__|__|,|__|__|__|
l) Personal care – toilet paper, toothpaste, hair products, razor, perfumes, lotions, make-up, beauty salons (exclude toilet/laundry soap)	|__|,|__|__|__|,|__|__|__|
m) Sports, theaters, and other forms of entertainment	|__|,|__|__|__|,|__|__|__|
n) Expenses in hotels and other forms of lodging	|__|,|__|__|__|,|__|__|__|
o) Sports betting and other forms of gambling	|__|,|__|__|__|,|__|__|__|

Q56: Now I will read you another list of items. For each item or expense, please tell me how much your household spent on the item **in the past 12 months**.

Item	*USH*
a) Clothes/shoes/material for adult males over the age of 18	|__|,|__|__|__|,|__|__|__|
b) Clothes/shoes/material for adult females over the age of 18	|__|,|__|__|__|,|__|__|__|
c) Clothes/shoes/material for boys under the age of 18 (excluding school uniforms)	|__|,|__|__|__|,|__|__|__|
d) Clothes/shoes/material for girls under the age of 18 (excluding school uniforms)	|__|,|__|__|__|,|__|__|__|
e) Modern medical treatment and medicines	|__|,|__|__|__|,|__|__|__|
f) Traditional medical treatment and medicines	|__|,|__|__|__|,|__|__|__|
g) School fees	|__|,|__|__|__|,|__|__|__|
h) Other educational expenses (exercise books, pens, pencils, uniforms, maintenance, club fees, etc.)	|__|,|__|__|__|,|__|__|__|
i) Cooking items/utensils, such as pots (except coal pot), pans, buckets, mortar, pepper grinder, grater, strainer, etc.	|__|,|__|__|__|,|__|__|__|
j) Donations to the church or mosque	|__|,|__|__|__|,|__|__|__|
k) Electricity	|__|,|__|__|__|,|__|__|__|
l) Water charges	|__|,|__|__|__|,|__|__|__|
m) Home improvements	|__|,|__|__|__|,|__|__|__|
n) Club membership fees (unions, Rotary, social clubs, traditional groupings)	|__|,|__|__|__|,|__|__|__|
o) Payments to domestic servants (security guards, cleaner, laundry person)	|__|,|__|__|__|,|__|__|__|

	Read: *Thank you. Now I would like to ask you some general questions on how money issues are handled in your household. Please remember that your answers are strictly confidential and only used for research purposes.*	
Q57:	If someone has some money but wants to make sure he does not spend it, what is the best way for him to put it? *CIRCLE ONE*	POCKET---------- 01 TIN WHERE HE/SHE STAYS---------- 02 HIDDEN AT HOME STAY (EX. IN MATTRESS) ---------- 03 IN A HOLE IN GARDEN---------- 04 ROTATING SAVINGS CLUB (ROSCA) ---------- 05 SACCO---------- 06 GROUP ACCOUNT AT A FORMAL BANK---------- 07 INDIVIDUAL ACCOUNT AT A FORMAL BANK---------- 08 HAVE ANOTHER PERSON (E.G. A FRIEND, MY MOTHER) HOLD IT FOR HIM/HER---------- 09 BUY THINGS THAT HE/SHE CAN SELL IF HE NEEDS TO (SUCH AS A GOAT OR A BICYCLE) ----------10
Q58:	Are you involved in making decisions about how to use money in your household? *READ ALL RESPONSE OPTIONS – CIRCLE ONE*	Yes, you make all financial decisions alone---------- 1 Yes, you are involved in all financial decisions in the household---------- ---2 Yes, you are involved in SOME financial decisions, but not all-----3 No, you are not involved in financial decisions---------- 4

Read: Thank you. Now I would like to ask you about any groups in which you are involved. These include formal groups that have meetings, but also informal groups where you happen to meet with someone unannounced.

Q59: How many community groups are you a currently a member of, participate in, interact with, or volunteer for?	__ __ groups If ***zero***, go to **Q 63**
Read: Now think about the group you are meeting with most often	
Q60: Are financial matters an important topic in your group meetings and activities?	1. Often 3. Rarely 2. Sometimes 4. Never
Q61: How many times in the last 30 days have you discussed about money as a group in a meeting?	[_____] times *DON'T KNOW: -98*
Q62: How many times in the last 30 days have you discussed personal money issues with another person who is in your group? This does not necessarily have to be for a group event, it could just be asking advice from a fellow group member about money issues.	[_____] times *DON'T KNOW: -98*

6 BUDGETING MODULE		
Q63:	Do you regularly keep track of how much money you spend?	YES---------- 1 NO ---------- 2 → ***TO Q 68***
Q64:	How do you usually keep track of how much money you spend? *READ ALL RESPONSE OPTIONS - CIRCLE ONE!*	Write it down ---------- 1 Someone else writes it down for you ---------- 2 Make a mental plan ---------- 3 Other: |_____|---------- 9
Q65:	How many times in the last 6 months have you done this activity in order to keep track of the money you spend? *HELP THE RESPONDENT ESTIMATE*	|___|___|times
Q66:	In the last 6 months, how many times has your plan failed, either because you got less money than you expected or because you had to spend more money than you expected?	|___|___|times ***IF 0 → TO Q68***

Q67:	What did you do when your plan failed? *READ ALL RESPONSE OPTIONS* *CHOOSE ALL THAT APPLY!*	Borrowed money ---------- 1 Sold something that you owned ---------- 2 Found some other work to do in order to make more money ---------- 3 Spent less on the things that you did not need as much ---------- 4 Did nothing ---------- 5 Other |_____|----- 9
Q68:	Do you keep separate records for your business inflows and outflows and your personal finances?	1. Yes 2. No
Q69:	Which of the following are included in a budget? <u>READ OUT ALL OPTIONS!</u> *CIRCLE ALL THAT THE RESPONDENT THINK IS INCLUDED*	A) Expected income B) Last month's income C) Expected expenditure D) Last month's expenditure E) Planned savings F) I'm not sure

7 PAYMENTS MODULE

Q70:	Have you ever made a payment using your mobile phone?	1 Yes 2 no → ***Q74***
Q71:	How often do you make payments using your mobile phone?	___ times per month
Q72:	Why do you make payments using your mobile phone? *CIRCLE ALL THAT APPLY*	1 required by partner/ receiver 2 safety 3 convenience 4 all of above 99 Other
Q73:	In which context do you use your phone to make payments? *CIRCLE ALL THAT APPLY*	1 private / family 2 business 3 Both 99 Other

8 INSURANCE MODULE

Q74:	Have you ever purchased an insurance product?	1 Yes 2 No → ***Q 78***
Q75:	Which kind of insurance product have you purchased?	1. Motor vehicle, 2. loan, 3. death/accident, *4.* other *(please specify)* _____/
Q76:	Are you currently covered by any kind of insurance?	1 Yes 2 No → ***Q 78***
Q77:	If yes, which insurance?	1. Motor vehicle, 2. loan, 3. death/accident, 4. other *(please specify)* _____/

10 FINANCIAL LITERACY MODULE

ENUMERATOR, PLEASE READ OUT THE QUESTIONS AS THEY ARE WRITTEN AND CIRCLE THE ANSWER GIVEN BY THE PARTICIPANT. DO NOT ASSIST THE PARTICIPANT IN ANSWERING THE QUESTIONS AND DO NOT EXPLAIN WHAT THE CONCEPTS REFERED TO IN THE QUESTIONS MEAN.

Q78:	Suppose you borrow 100,000 UGX at an interest rate of 2% per month, with no repayment for 3 months. After 3 months, do you owe A) less than. 102,000 UGX, Y) DON'T KNOW B) exactly. 102,000 UGX , Z) REFUSE TO ANSWER C) or more than 102,000 UGX? ***(PLEASE CIRCLE THE RESPONSE***)
Q79:	If you have UGX. 100,000 in a savings account earning 1% interest per annum, and prices for goods and services rise 2% over a 1-year period, can you buy A) more than, Y) DON'T KNOW B) less than, Z) REFUSE TO ANSWER C) or the same amount of goods in 1 year as you could today, with the money in the account?" ***(PLEASE CIRCLE THE RESPONSE***)

Q80:	Is it riskier to plant A) multiple crops or B) one crop? Y) DON'T KNOW Z) REFUSE TO ANSWER ***(PLEASE CIRCLE THE RESPONSE***)
Q81:	Suppose you need to borrow. 500,000 UGX. Two people offer you a loan. Which loan represents a better deal for you? A) One loan requires you to pay back 600,000 UGX in 1 month. B) The second loan requires you to pay back in 1 month 500,000 UGX plus 15% interest. Y) DON'T KNOW Z) REFUSE TO ANSWER ***(PLEASE CIRCLE THE RESPONSE***)
Q82:	Suppose you owe 3,000,000 UGX to a bank. You pay a minimum payment of 30,000 UGX each month. At an Annual Percentage Rate of 12% (or 1% per month), how many years would it take to eliminate debt if you took no additional loan? A) Less than 5 years B) Between 5 and 10 years C) Between 10 and 15 years; D) Never, you will continue to be in debt; Y) DON'T KNOW Z) REFUSE TO ANSWER ***(PLEASE CIRCLE THE RESPONSE)***
Q83:	If you were offered a loan with 5% monthly interest rate and a loan with 20% annual interest rate, which loan would offer better value? A) 5% monthly interest rate B) 20% annual interest rate Y) DON'T KNOW Z) REFUSE TO ANSWER (***PLEASE CIRCLE THE RESPONSE)***

11 NUMERACY MODULE		
***READ**:* These next questions ask you to do some math in your mind. Remember that this is not a test, so it doesn't matter if you are right or wrong! *NOTE: FOR THE QUESTIONS 84 to 86 "A WHILE" MEANS ABOUT 10 SECONDS, BUT YOU DO NOT NEED TO KEEP TIME – JUST ESTIMATE WHETHER THE RESPONDENT TOOK MORE OR LESS THAN 10 SECONDS TO ANSWER. AS MUCH AS POSSIBLE, DO NOT ALLOW THE RESPONDENT*		
Q84:	What does 16 plus 12 equal?	GIVES RIGHT ANSWER (28) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ---------- 99
Q85:	Imagine that five brothers are given a gift of 10,000 UGX. If the brothers have to share the money equally, how much does each one get?	GIVES RIGHT ANSWER (2,000UGX) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ----------99
Q86:	Suppose you put 1,000 UGX into a free-of-charge savings account with a guaranteed interest rate of 10% per year. How much would be in the account at the end of the first year, once the interest payment is made?	GIVES RIGHT ANSWER (1,100UGX) QUICKLY ---------- 1 GIVES RIGHT ANSWER AFTER A WHILE ---------- 2 GIVES WRONG ANSWER: |___|---------- 3 DOES NOT TRY TO ANSWER ----------99
Q87:	If the same bicycle is on sale in two different shops at 200,000 shillings and one shop offers a discount of 30,000 shillings and the other shop offers a 10% discount: which one is the better	A discount of 30,000 UGX _____ 1 A discount of 10% _____ 2 Don't know_____ 99
Q88:	If you have 4,800 UGX and friend gives you 5,800 UGX, how many UGX do you have?	_____UGX Don't know_____ 99

12 SAVING BEHAVIOR MODULE		
Q89:	Do you have any money saved? Just to clarify, savings do not have to be deposited in an account or formal institution, and they may or may not gain interest. They can be somewhere at home, hidden in a safe place, or with a friend or family member.	YES---------- 1 → ***TO Q91a*** NO ----------2
Q90:	So there is no place where you are saving your money right now?	HAS SAVINGS ---------- 1 NO SAVINGS ----------2 → ***TO Q105***

READ: We are interested in learning more about market vendors' savings behavior and access to financial services, so in this next section we will ask about where and how you save money. Please remember that this information is confidential – no one other than the researchers will see this information, and your name will not be connected to it.

READ: I would like to know all of the ways and places you save money, and then I will ask you a couple more questions about each of these places or ways. Can you please tell me the places or ways that you save money right now?

HAVE THE RESPONDENT LIST OFF THE PLACES WHERE HE/SHE HAS MONEY SAVED, IN ORDER FROM MOST TO LEAST MONEY. CODE THE LOCATIONS IN EACH ROW OF THE FIRST COLUMN (Q91a "PLACE CODE") IN THE BELOW TABLE ACCORDING TO THE BELOW CODES, THEN PROCEED WITH QUESTIONS 91b – 102 FOR EACH LOCATION.

01) POCKET
02) IN A "LOCAL BANK", BOX OR TIN AT HOME
03) HIDDEN AT HOME (EX. "IN MY MATTRESS")
04) IN A HOLE IN THE GARDEN
05) IN A ROTATING SAVINGS CLUB (ROSCA)
06) IN A SACCO
07) IN A TELECOM COMPANY ACCOUNT (EX. MTN MOBILE MONEY, UTL M-SENTE, ZAIN ZAP)
08) IN A GROUP ACCOUNT AT A FORMAL BANK
09) IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK
10) ANOTHER PERSON (E.G. A FRIEND, MY MOTHER) HOLDS IT
11) SAVES BY BUYING THINGS THAT CAN BE RESOLD (SUCH AS A GOAT OR A BICYCLE)
12) BY INVESTING IN MY BUSINESS
99) OTHER

	Q91:a Place **CODE**	**Q91: b** How much money do you have saved in […]?	**Q92:** How many months ago did you first start saving in […]? IF LESS THAN 1 MONTH = 0	**Q93:** How many weeks ago did you last put money in […]? IF LESS THAN 1 WEEK = 0	**Q94:** How much money did you put in […] that time? REFERRING TO THE TIME IN Q93	**Q95:** How often do you put money in […]? READ RESPONSE OPTIONS 1) Often 2) Sometimes 3) Rarely 4) Never	**Q96:** How many weeks ago did you last take money out of […]? WITHIN LAST WEEK = 0 NEVER = 99 →**Q98**	**Q97:** How much money did you take out of […]? REFERRING TO THE TIME IN Q96	**Q98:** How often do you take money out of […]? READ RESPONSE OPTIONS 1) Often 2) Sometimes 3) Rarely 4) Never	**Q99:** Why do you choose […] to keep your money in? A) NO CLEAR REASON B) SAFETY/SECURITY C) EASY TO ACCESS D) DIFFICULT TO ACCESS SO WON'T SPEND E) NOT ENOUGH MONEY FOR FORMAL BANK ACCOUNT F) EARNS MONEY (E.G., INTEREST; ITEM WHICH CAN BE RESOLD FOR MORE) G) SOMEONE ELSE TOLD YOU TO H) FREE TO SAVE (NO CHARGES) Z) OTHER REASON	**Q100:** Are you saving up money in this place in order to achieve a certain plan? 1) Yes 0) No ***If 1→ NEXT ROW***	**Q101:** What is your plan or goal for what you will do with the money in […]? A) LARGE PURCHASE (INCLUDING LAND AND BUILDING A HOUSE) B) SAVE FOR EMERGENCIES C) USE TO INVEST OR PAY FOR SOMETHING THAT WILL EARN MORE MONEY D) EDUCATION COSTS FOR SELF OR OTHER E) HEALTH-RELATED COSTS FOR SELF OR OTHER Z) OTHER	**Q102:** How much money **total** do you want to save in order to achieve this plan or goal? DON`T KNOW = -99
		UGX	*# of MONTHS*	*# of WEEKS*	*UGX*	*CIRCLE ONE*	*# of WEEKS*	*UGX*	*CIRCLE ONE*	*CIRCLE ALL THAT APPLY*	*CIRCLE ONE*	*CIRCLE ALL THAT APPLY*	*UGX*
1	|__|	|__|,|__|__|__|,|__|__|__|			|__|,|__|__|__|,|__|__|__|	1 2 3 4		|__|,|__|__|__|,|__|__|__|	1 2 3 4	A B C D E F G H Z	*1…0*	A B C D E Z	|__|,|__|__|__|,|__|__|__|
2	|__|	|__|,|__|__|__|,|__|__|__|			|__|,|__|__|__|,|__|__|__|	1 2 3 4		|__|,|__|__|__|,|__|__|__|	1 2 3 4	A B C D E F G H Z	*1…0*	A B C D E Z	|__|,|__|__|__|,|__|__|__|
3	|__|	|__|,|__|__|__|,|__|__|__|			|__|,|__|__|__|,|__|__|__|	1 2 3 4		|__|,|__|__|__|,|__|__|__|	1 2 3 4	A B C D E F G H Z	*1…0*	A B C D E Z	|__|,|__|__|__|,|__|__|__|
4	|__|	|__|,|__|__|__|,|__|__|__|			|__|,|__|__|__|,|__|__|__|	1 2 3 4		|__|,|__|__|__|,|__|__|__|	1 2 3 4	A B C D E F G H Z	*1…0*	A B C D E Z	|__|,|__|__|__|,|__|__|__|
5	|__|	|__|,|__|__|__|,|__|__|__|			|__|,|__|__|__|,|__|__|__|	1 2 3 4		|__|,|__|__|__|,|__|__|__|	1 2 3 4	A B C D E F G H Z	*1…0*	A B C D E Z	|__|,|__|__|__|,|__|__|__|

Q103:	*DO NOT READ:* CHECK QUESTION 91a: DID THE RESPONDENT LIST 8) "IN A GROUP ACCOUNT AT A FORMAL BANK" OR 9) "IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK"?	YES----------1 → ***TO Q 105*** NO ----------2
Q104:	*ONLY ASK IF DID NOT LIST 8) "IN A GROUP ACCOUNT AT A FORMAL BANK" OR 9) "IN AN INDIVIDUAL ACCOUNT AT A FORMAL BANK" FOR Q91a:* Why do you choose not to have an account in a formal bank? *DO NOT PROBE. (SELECT ALL MENTIONED))*	DOES NOT HAVE ENOUGH MONEY TO OPEN ACCOUNT---------- 1 BANK IS TOO FAR AWAY---------- 2 BANKS ARE NOT SAFE OR SECURE (E.G. MAY BE ROBBED, BURN DOWN) ---------- 3 BANKS ARE NOT TRUSTWORTHY---------- 4 TOO MUCH WORK/TOO DIFFICULT TO OPEN ACCOUNT---------- 5 BANK FEES/CHARGES ARE TOO HIGH……6 DOES NOT KNOW---------- 98

13 BORROWING MODULE

Q105:	Do you think you could be able to borrow 100,000 UGX in case you want to?	YES---------- 1 NO ---------- 2 →***to 107***
Q106:	From where do you think it is most likely you could borrow this money from? *CHOOSE ONE*	FAMILY MEMBER OR FRIEND----------1 COMMERCIAL BANK----------2 SAVINGS GROUP ----------3 MICROFINANCE AGENCY---------- 4 MONEYLENDER---------- 5 OTHER (specify)|_____|99
Q107:	Do you think you could be able to get a loan of 1 million UGX in case you want to?	YES---------- 1 NO ---------- 2→ ***TO 109***
Q108:	From where do you think it is most likely for you to obtain such a loan? *CHOOSE ONE*	FAMILY MEMBER OR FRIEND---------- 1 COMMERCIAL BANK---------- 2 SAVINGS GROUP ---------- 3 MICROFINANCE AGENCY---------- 4 MONEYLENDER---------- 5 OTHER (please specify) _____ 99
Q109:	In the past 5 years, did it ever happen to you that you were late with any payment you owed (to a bank or moneylender or relative)?	YES---------- 1 NO ---------- 2 →***TO 111***
Q110:	How often did it happen to you that you were late with a payment in the past 5 years?	ONCE _____ ___1 TWICE _____2 BETWEEN 3 AND 5 TIMES _____ 3 MORE THAN 5 TIMES _____ 4
Q111:	Have you ever applied for a loan and have been rejected?	NEVER_____0 ONCE _____ 1 TWICE _____2 BETWEEN 3 AND 5 TIMES __3 MORE THAN 5 TIMES _____ 4
Q112:	In general, how interested are you in financial matters?	1 …Not interested 2… interested 3… very interested

READ: Now I would like to ask you some questions about lending and borrowing money. First let's discuss money that you borrowed, also known as loans. We define loans as money you received that you have to repay. Loans can come from a formal source such as a bank or microfinance institution, or from an informal source such as a friend, relative, money lender or local savings group.

Q113: In the past 6 months, have you borrowed money that you are or will be expected to pay back?

YES ---------- 01

NO ---------- 02 → ***TO Q125***

Please list all of the people or places you borrowed a lot of money from in the **past 6 months**?

COMPLETE TABLE BELOW FOR EACH SOURCE OF LOAN

	Q114: NAME OF SOURCE	**Q115:** CODE OF SOURCE 1)Family member or friend 2) Other Market Vendor 3) Commercial bank 4) Savings group 5) Microfinance agency 6) Moneylender 7) Workplace 9) Other	**Q116:** How many months ago did you last borrow from […]? IF LESS THAN 1 month = 0 DON'T KNOW= -99	**Q117:** How much money did you borrow from […]? DON'T KNOW= -99	**Q118:** How much money do you still need to pay back?	**Q119:** Did you or will you have to pay back more money than you borrowed? 1) Yes 2) No → to **Q 124**	**Q120:** For the extra money you will have to pay, do you know the interest rate or do you know the actual amount you have to pay? 1) Interest→ next **Q121** 2)Extra amount → to **Q123**	**Q121:** What is the interest rate you paid on the money you borrowed?	**Q122:** Is this interest rate per year, month, week or day? 1) Year 2) Month 3) Week 4) Day 5) Other → to **Q124**	**Q123:** What is the extra amount of money you will have to pay on top of what you borrowed?	**Q124:** What is the main thing you used the money for? 1) Pay for emergency 2) invest in own business or farm 3) Pay primary or secondary school fees for self 4) Pay primary or secondary school fees for other person 5) Pay for university 6) Invest in other's business or farm 7) Buying non-essential things (luxury or enjoyment) 5) Basic needs (food, shelter or preventative medical costs) 9) Other
		ENTER ONE	# of MONTHS	*UGX*	*UGX*	*CIRCLE ONE*	*CIRCLE ONE*	*%*	*CIRCLE ONE*	*UGX*	*CIRCLE ONE*
1				|__|,|__|__|__|,|__|__|__|	|__|,|__|__|__|,|__|__|__|	*1 2*	*1 2*		*1 2 3 4 5*	|__|,|__|__|__|,|__|__|__|	*1 2 3 4 5 6 7 8 9*
2				|__|,|__|__|__|,|__|__|__|	|__|,|__|__|__|,|__|__|__|	*1 2*	*1 2*		*1 2 3 4 5*	|__|,|__|__|__|,|__|__|__|	*1 2 3 4 5 6 7 8 9*
3				|__|,|__|__|__|,|__|__|__|	|__|,|__|__|__|,|__|__|__|	*1 2*	*1 2*		*1 2 3 4 5*	|__|,|__|__|__|,|__|__|__|	*1 2 3 4 5 6 7 8 9*
4				|__|,|__|__|__|,|__|__|__|	|__|,|__|__|__|,|__|__|__|	*1 2*	*1 2*		*1 2 3 4 5*	|__|,|__|__|__|,|__|__|__|	*1 2 3 4 5 6 7 8 9*
5				|__|,|__|__|__|,|__|__|__|	|__|,|__|__|__|,|__|__|__|	*1 2*	*1 2*		*1 2 3 4 5*	|__|,|__|__|__|,|__|__|__|	*1 2 3 4 5 6 7 8 9*

14 LENDING MODULE

Q125:	In the past **6 months**, have you given a loan to anybody, with the expectation that they pay you back?	YES---------- 1 NO ---------- 2→ ***TO Q133***

Please list all of the people you lent money to in the past **6 months**

COMPLETE TABLE BELOW FOR EACH BORROWER

	Q126:	**Q127:**	**Q128:**	**Q129:**	**Q130:**	**Q131:**	**Q132:**
	BORROWER NAME	BORROWER CODE 1) Family member or friend 2) Client/customer 9) Other *CIRCLE ONE*	How much did you lend to this person? *UGX*	How many months ago did you lend money to […]? *WITHIN LAST MONTH = 0* *DON'T KNOW = 98*	Did they or will they pay back more money than they borrowed? 1) Yes 2) No ***TO Q132*** *CIRCLE ONE*	What interest rate will they have to pay back to you? *DO NOT PROBE OR HELP THE RESPONDENT CALCULATE* *DON'T KNOW = 998* *%*	How much money are you still waiting to receive back? *DON'T KNOW = 998* *UGX*
1		1 2 9	|__|,|__|__|__|,|__|__|__|	|___|	1 2		|__|,|__|__|__|,|__|__|__|
2		1 2 9	|__|,|__|__|__|,|__|__|__|	|___|	1 2		|__|,|__|__|__|,|__|__|__|
3		1 2 9	|__|,|__|__|__|,|__|__|__|	|___|	1 2		|__|,|__|__|__|,|__|__|__|
4		1 2 9	|__|,|__|__|__|,|__|__|__|	|___|	1 2		|__|,|__|__|__|,|__|__|__|
5		1 2 9	|__|,|__|__|__|,|__|__|__|	|___|	1 2		|__|,|__|__|__|,|__|__|__|

15 ACCESS TO LUMP SUM MODULE

Read: Thanks again for your time! Next I'd like to ask a couple questions about times you've had to spend a large amount of money

Q133:	How many months ago was the last time you had an emergency that required for you to pay your own money (such as a burial, a fire or a family member or friend falling sick)? *WITHIN THE PAST MONTH = 00* *NEVER = 99*	|___|___|months ***IF NEVER (99)→ TO Q 136***
Q134:	How much did you have to pay?	|___|___|,|___|___|___|,|___|___|___|UGX
Q135:	Where did you get the money from that you used to pay for that emergency? *MARK ALL THAT APPLY*	BORROWED---------- A USED SAVINGS---------- B SOLD SOMETHING OF YOURS---------- C GIVEN MONEY BY ANOTHER PERSON---------- D OTHER: |_____|--- Z
Q136:	How many months ago was the last time you purchased something that cost a lot of money? *WITHIN THE PAST MONTH = 00* *NEVER = 99*	|___|___|months ***IF NEVER (99)→ TO Q139***
Q137:	How much did it cost?	|___|___|,|___|___|___|,|___|___|___|UGX
Q138:	Where did you get the money from that you used to pay for that thing? *MARK ALL THAT APPLY*	BORROWED---------- A USED SAVINGS---------- B SOLD SOMETHING OF YOURS---------- C GIVEN MONEY BY ANOTHER PERSON---------- D OTHER: |_____|--- Z

16 INVESTMENT BEHAVIOR MODULE		
Q139:	How much of your own money did you spend on investments in business in order to try to make profits in the **past 6 months**? It is okay to estimate. *PROBE. EMPHASIZE THAT YOU WANT TO KNOW TOTAL FOR ALL 6 MONTHS* *DON'T KNOW = -99*	|___|___|,|___|___|___|,|___|___|___|UGX
Q140:	How did you get <u>most</u> of the money for these business-related expenses? *READ ALL RESPONSE OPTIONS* *CIRCLE ONLY <u>ONE</u>!!!*	Borrowed from somewhere that you will have to pay more money back to -- 1 Borrowed from somewhere that you will have to pay the same amount of money back to ---------- 2 Given money from someone else ---------- --3 Did some additional work ---------- 4 Money you already had/ savings ---------- 5 Sold something that you owned ---------- 6 Other (specify) _____8
17 BIAS AND PREFERENCES MODULE		
Read: Now I would like to ask you a few questions about your behaviors and preferences. I will read out a few questions to you.		
Read: For these next 2 questions, please answer either: often, sometimes, rarely or never. Again remember there is no right or wrong answer!		
Q141:	If you get money, do you tend to spend it too quickly?	OFTEN---------- 1 SOMETIMES ---------- 2 RARELY---------- 3 NEVER---------- 4→ ***TO Q143***
Q142:	Do you therefore put most of your money into a safe place in order to avoid spending it too quickly?	OFTEN---------- 1 SOMETIMES ---------- 2 RARELY---------- 3 NEVER---------- 4

Q143:	Are you generally a person who is fully prepared to take risks or do you try to avoid taking risk? (Please choose a number on a scale from 0 to 10)	0 1 2 3 4 5 6 7 8 9 10 (try to avoid risk) (Fully prepared to take risk)
Q144:	Attitudes towards risk change in different situations. When thinking about investing and borrowing, are you a person who if fully prepared to take risks or do you try to avoid taking risk? (Please choose a number on a scale from 0 to 10)	0 1 2 3 4 5 6 7 8 9 10 (try to avoid risk) (Fully prepared to take risk)
Read: Thank you. For the next two questions, you will be asked which of two options you prefer more.		
Q145:	Imagine you just won 100 000 UGX in a lottery and you can invest this money in a business. There is a 50 % chance that the business is successful. If the business is successful you double the amount invested after one year. If it is not successful you will lose half the amount you invested. What fraction of the 100 000 UGX would you invest in the business?	|__|__|__|,|__|__|__|UGX
Q146:	Suppose you have some money to do business, and you have the choice between two options: **Option A**: A business that can give a lot of profit every month, but there is a chance you can lose your money anytime. **Option B**: A business with less profit every month, but you can't lose your money. Which option would you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 98

18 GiZ AND BoU-MODULE		
Q147:	Do you think the following statements are true or false?	
a)	If someone buys livestock, it is only an investment if that person intends to obtain future income from it.	True _____ 1 False _____ 2

b)	When you buy on credit the goods end up being more expensive than they would be if bought on cash.	True _____ 1 False _____ 2	
Q148:	How safe do you consider the following places for saving money?		
a)	Regulated Financial Institutions (Banks, MDIs, Credit Institutions)	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99	
b)	SACCO	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99	
c)	VSLA	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99	
d)	At home	Not safe _____ 1 Somewhat safe ____ 2 Very safe _____ 3 Don't know _____ 99	
Q149:	Under which of the following circumstances would you consider a loan as bad? *PROBE ALL ANSWERS!* *CIRCLE ALL THAT THE RESPONDENT THINKS APPLIES*	a) No clear plan of how the money will be used b) No plan on how the money will be paid back c) The money is used for the intended purpose d) The money is used for paying back another debt e) The money is used for luxuries f) The money is used for every-day expenses g) The money is used for productive investment h) Borrowing because others are borrowing	
Q150:	If you were offered a loan but you were not sure what you would use it for, would you still take it?	Yes _____ 1 No _____ 0 Don't know _ 99	
Q151:	In case you are dissatisfied with a financial service provider and you complain, do you think that the financial service provider is more powerful than you, and that the complaint will therefore not lead to anything?	Yes _____ 1 No _____ 0 Don't know _ 99	
Q152:	Please tell me how strongly you agree or disagree with the following statements, which other people have made about taking out financial products		
a)	I am confident enough to approach a bank and ask questions to learn more about their products.	Disagree strongly ___ 1 Tend to agree _____ 2 Agree strongly _____ 3 Don't know _____ 99	
b)	I am confident that among a range of loans offered by different banks, I can choose the loan that best suits my specific needs.	Disagree strongly ___ 1 Tend to agree _____ 2 Agree strongly _____ 3 Don't know _____ 99	
Q153:	Which of the following statements best describes how you last chose a financial product (loan, account, policy…)? *CHOOSE ONE!!!*	I considered several products from different companies before making my decision _____ 1 I considered the various products from one company _____ 2 I didn't consider any other products at all _____ 3 I looked around but there were no other products to consider ___ 4	
	Have you heard of any of the following financial products? If so, do you currently hold or use any of these types of products (personally or jointly)?		
Q154:	Bank loan	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q155:	Microfinance loan	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q156:	Mortgage	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q157:	Savings account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q158:	Current account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0

Q159:	Fixed deposit account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q160:	Overdraft	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q161:	Pension fund	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q162:	Insurance	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q163:	Cheque book	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q164:	Credit card	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q165:	ATM card / debit card	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q166:	Mobile money account	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q167:	Cell phone banking (with a bank account)	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0
Q168:	Money transfer services (Western Union, Money Gram)	a) Heard of it Yes:1 No: 0	b) Hold or use it Yes:1 No: 0

19 PARTICIPATION IN OTHER PROGRAMS	___
Read: Now I want us to talk about programs that you or your community received in the past from CBOs, NGOS or the government..	
Q169: Aside from our visit, did you receive help from any other program since we last visited you?	1. Yes 0. No → **Q173**
Q170: What was the program?	1. Government 2. NGO 3. CBO 96. Other
Q171: Fill in the name of the program:	|_____|
Q172: What did you receive?	a. Cash b. Training c. Counseling d. Medical aid e. Other
Q173: Have you heard about the national strategy for financial literacy in Uganda?	1. yes 0. no
Q174: Have you ever seen TV-ads or ads in newspapers by the Bank of Uganda concerning issues on money management?	1. yes 0. no→ **Q176**
Q175: From your memory, how often have you seen these TV or newspaper-ads?	__times
Q176: Have you ever seen TV-ads or ads in newspapers by other private institutions concerning issues on personal money management?	1. yes 0. no →**Q178**
Q177: From your memory, how often have you seen these TV or newspaper-ads?	__times

20 RISK -PREFERENCES MODULE		
***Read*:** We are almost done with the interview, and we appreciate your patience. In these last few questions, we will play a fun exercise that will involve potential payouts with real money. One of these questions will be chosen to be actually be paid at the end of our session, so be careful about which option you choose for each question, since that one might be chosen for you to play to receive money, and if so you will not be able to change your answer! In this activity, we will ask you two types of questions in which you choose between different options. In the first questions, you will be asked to choose between games of zala in which you can win different amounts of money. Zala (or labyeka) is a child's activity some of you may remember. In the activity, I have a stone in one hand, and you do not know which hand. You must then guess the hand with the stone. If you guess correctly, you win. If you do not guess correctly, you lose. You will receive money based on your answers to 1 randomly selected question in the following exercise. Take care in the choices you make for all questions, because once you have answered all of the questions, we will reveal which question has been randomly selected to be performed with real money. We will then use the responses you have selected for those questions to determine the actual payment. You will not be able to change your responses once we reveal which questions have been selected. This is not our personal money. Rather, it is money given to us by the research organization, to do this activity in order to better understand your preferences. Do you have any questions before we proceed? *ANSWER ANY QUESTIONS*.		
Q178:	**(1)** Imagine you have a choice between the following two options: Option A: You can receive **900** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q179:	**(2)** Now imagine you have a choice between the following two options: Option A: You can receive **600** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q180:	**(3)** Now imagine you have a choice between the following two options: Option A: You can receive **1,200** USH for sure Option B: We play zala. If you win you get **1,500** USH. If you lose, you get **500** USH. Which option do you choose?	OPTION A---------- 1 OPTION B---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q181:	**(4)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **3,000** Shillings. If you lose, you get **2000** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **1000** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q182:	**(5)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **2,000** Shillings. If you lose, you get **1500** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **1000** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Q183:	**(6)** Now imagine you have a choice between playing two different games of zala. Game 1: We play zala. If you win, you get **3,000** Shillings. If you lose, you get **2000** Shillings. Game 2: We play zala. If you win, you get **5,000** Shillings. If you lose, you get **0** Shillings. Which game would you choose to play?	GAME 1---------- 1 GAME 2---------- 2 NO PREFERENCE---------- 3 DON'T KNOW---------- 8
Read: Thank you very much for your participation. Now we are going to play one of the games above for real money. Which of the games we play is determined randomly by throwing a dice. *ENUMERATOR: THROW A DICE (**ONLY ONCE**!). THE NUMBER ON THE DICE INDICATES WHICH OF THE 6 GAMES ABOVE (MARKED WITH (1) TO (6) AT THE BEGINNING OF THE QUESTION WILL BE PLAYED. WRITE DOWN WHICH GAME WAS CHOSEN BELOW AT Q184 (NUMBER ON DICE). THEN CHECK WHICH OPTION OR GAME THE PARTICIPANT CHOSE FOR THIS QUESTION. IF THE PARTICIPANT CHOSE THE SURE AMOUNT, TELL HIM THAT HE CHOSE THE SURE AMOUNT AND WRITE DOWN THE AMOUNT IN THE FIELD BELOW (Q185).* *IF THE PARTICIPANT CHOSE THE GAME, TELL HIM THAT YOU ARE GOING TO PLAY A ROUND OF ZALA/ LABYEKA NOW. THEN PLAY ONE ROUND (**AND ONLY ONE** - DO NOT REPEAT THE GAME!) IF THE PARTICIPANT WINS, CHECK IN THE DESCRIPTION OF THE GAME ABOVE HOW MUCH HE*		

WINS, TELL HIM HOW MUCH IT IS AND WRITE DOWN THE AMOUNT HE WINS BELOW. IF HE LOSES, CHECK HOW MUCH MONEY THE PARTICIPANT RECEIVES IN THE DESCRIPTION OF THE GAME ABOVE, TELL THE PARTICIPANT HOW MUCH HE RECEIVES AND WRITE DOWN THE AMOUNT BELOW. IF THE RESPONDENT CHOSE "NO PREFERENCE", ROLE THE DICE AGAIN ONCE. IF THE DICE SHOWS NUMBER 1,2 OR 3, PLAY OPTION A OF THE GAME AS DESCRIBED ABOVE. IF THE DICE SHOWS NUMBER 4, 5 OR 6, PLAY OPTION B OF THE GAME AS JUST DESCRIBED ABOVE.		
Q184:	Number on the dice	_____
Q185:	Payment amount:	__ __ __ __ USH

21. RESPONDENT TRACKING INFORMATION MODULE

Remember we would like to conduct a similar survey with you about three/four months from now. At that time, you will again be free to decide you do not want to participate in the survey. We'd like to ask you some information about yourself and people you know so that we will be able to find when we return.

Q186:	Q What is your surname? *WRITE IN CLEAR BLOCK LETTERS,*	|_____|
Q187:	What is your first name? *CLEAR BLOCK LETTERS, CHECK*	|_____|
Q188:	What is your other name? *IF NO OTHER NAME, LEAVE BLANK*	|_____|
Q189:	What is your address?	|_____|
Q190:	What is the name of the village where you usually	|_____|
Q191:	What is this village's subcounty?	|_____|
Q192:	What is this village's parish?	|_____|
Q193:	What is/are your phone numbers? *PHONE 1*	0|__|__|__| - |__|__|__|__|__|__|__|
Q194:	*PHONE 2*	0|__|__|__| - |__|__|__|__|__|__|__|
Q195:	*PHONE 3*	0|__|__|__| - |__|__|__|__|__|__|__|
Q196:	Is there some way to describe where you live? (For example, near the old Catholic church or under the large mango tree on the hill)	

What are the names and contact information of two people who will likely know where you are that we can contact for information on next visit?

Q197:	*CONTACT 1 NAME*	|_____|	*CONTACT 1*
Q198:	*CONTACT 1 RELATIONSHIP TO RESPONDENT*	|_____|	
Q199:	*CONTACT 1 PHONE NUMBER*	0|__|__|__| - |__|__|__|__|__|__|__|	
Q200:	*CONTACT 1 ADDRESS*	|_____|	

Q201:	*CONTACT 2 NAME*	|_____|	*CONTACT 2*
Q202:	*CONTACT 2 RELATIONSHIP TO RESPONDENT*	|_____|	
Q203:	*CONTACT 2 PHONE NUMBER*	0|___|___|___| - |__|__|__|___|__|__|	
Q204:	*CONTACT 2 ADDRESS*	|_____|	

Read the following conclusion: We are now finished with the interview. Thank you very much for your time. The results of this survey will help interested parties understand how to better design decentralized development programs.		
Do you have any questions for me before I leave?	***Briefly answer any questions that the respondent asks, then complete the rest of the survey once the respondent has left.***	
Q205: Time interview ends	HH:MM: |___|___|:|___|___| AM/PM	
Q206: Result codes (*CIRCLE ONE):* 01. Completed 04. Partly completed 02. Postponed 05. Incapacitated 03. Refused 06. Other_____		
Q207*: Do not read:* ARE YOU SUSPICIOUS THAT THIS PERSON WAS INTOXICATED DURING THIS INTERVIEW?	1. No, completely sober 2. A little bit intoxicated 3. Somewhat intoxicated 4. Totally intoxicated	
Q208*: Do not read:* *HOW DID THIS PERSON'S THOUGHT PROCESS APPEAR TO YOU DURING THE SURVEY?*	1. Logical and sensible 2. A little unclear or confused 3. Several times unclear or insensible 4. Totally unclear or disoriented	
Q209*: Do not read:* *(a) Do you feel that this person needs to be referred for emergency assistance?*	1. Yes → ***Explain in part b and c*** 2. No	(b) For what reason? (c) How urgent? 1. Emergency (right away) 2. Moderate priority 3. Low priority
Notes		

Part II: Research on economic and social science education

(iii) Financial knowledge among future economics teachers – Results from a recent survey

Tim Kaiser and Vera Kirchner

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Abstract

Despite the increasing popularity of research on teacher professionalism in general, only few studies have examined specific professional knowledge among (prospective) teachers in economics. This study investigates the financial knowledge among prospective economics teachers in a novel survey employing the financial knowledge scale (Knoll and Houts 2012). The results indicate, that the respondents could – on average – solve 65.2% of the financial knowledge items correctly, but exhibit shortcomings in the content areas of investing and credit and borrowing. At the same time, we find a gap in subjective and objective financial knowledge ($r = 0.15$, $p < 0.05$), indicating poor abilities of self-assessment in our sample of future teachers. Multivariate analysis identifies gender as a predictor of financial knowledge where female respondents show test scores 5% lower than their male counterparts. This gender gap appears to be especially evident with regard to items that measure knowledge in the content areas of inflation and the minimization of risk through diversification. The results highlight the importance of further research in the area of teacher professionalism in economics and suggest the implementation of specific learning opportunities with regard to financial education in the context of teacher-training.

Keywords: financial literacy, financial knowledge, teacher professionalism

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[*]http://elibrary.steiner-verlag.de/das-finanzwissen-angehender-wirtschaftslehrpersonen-ergebnisse-eines-aktuellen-surveys.html

(iv) Student conceptions of regulatory policy issues – Results from a phenomenographic study

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Abstract

Being capable of thinking about and reflecting on economic systems and regulatory policies is a central aim of civic and economic education. In order to participate in the shaping of the social market economy in Germany it is necessary to deliberate about the balance of economic freedom and economic security through the provision of social protection aids. Active citizens are required to be capable of identifying and valuing conflicting societal goals and consequently of choosing between a set of competing regulatory policies. Against this backdrop the lack of empirical evidence regarding power of political judgment in reference to questions of regulatory policy is unfortunate. This paper reports on the methodological approach to and results from an explorative phenomenographic study on students political and economic reasoning. Our results show that students' answers to questions on regulatory policies can be categorized into six qualitatively distinct conceptions. Generally this empirical evidence suggests that students' views, reasoning and judgment on the choice of regulatory policies are often mono-perspectivistic and based upon unreflected ontological presuppositions.

Keywords: Conceptual change, social science education, economics education, phenomenography

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[†]http://www.wochenschau-verlag.de/ordnung-1525.html

Eidesstattliche Erklärung

Ich erkläre hiermit, dass ich meine Doktorarbeit „Essays on financial education and behavioral household finance“ selbstständig und ohne fremde Hilfe angefertigt habe und dass ich als Koautor maßgeblich zu den weiteren Fachartikeln beigetragen habe. Alle von anderen Autoren wörtlich übernommenen Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehnenden Ausführungen der aufgeführten Beitrge wurden besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert.

Kiel, den 13.03.2017

Tim Kaiser